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## COI Stories: Explanation and Evidence in the History of Science

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Dedicated to the memory of  
Wesley C. Salmon (1925–2001)

“... a stamp of truth beyond the power of ingenuity to counterfeit”  
—William Whewell

*This paper takes as its point of departure two striking incongruities between scientific practice and trends in modern history and philosophy of science.*

*(1) Many modern historians of science are so preoccupied with local scientific practices that they fail to recognize important non-local elements. (2) Many modern philosophers of science make a sharp distinction between explanation and evidence, whereas in scientific practice explanatory power is routinely used as evidence for scientific claims. I draw attention to one specific way in*

Three of my teachers at the University of Pittsburgh—John Norton and Wesley and Merrilee Salmon—planted the seed of COI in my head and taught me how to do philosophy of science in the first place. Gerd Graßhoff let me talk COI in his WTWG-unit (*Wissenschaftstheorie und Wissenschaftsgeschichte*) at the *Universität Bern*. We have been partners in crime pushing COIs on corruptible youths ever since. Bill Unruh and Catherine Wilson let me talk COI at the University of British Columbia in Vancouver. Lee Gohlike and Roger Stuewer invited me to the last three Seven Pines Symposia where I talked COI with various other participants. Friedrich Steinle invited me to his party in Denver in 2001, where I was able to hash things out with our co-panelist Ofer Gal. George Smith and Laura Snyder attended our session and made invaluable comments on earlier drafts of this paper. Further research was done in the stimulating environment of Jürgen Renn's *Abteilung* of the *Max-Planck-Institut für Wissenschaftsgeschichte*. Both participants in and visitors to my COI seminar at the University of Minnesota in the fall of 2002 made many valuable contributions. So did Tony Duncan. As will be clear from the footnotes I benefited from many people's advice. I want to single out Robert Schulmann, who decisively shaped my image of Einstein, and Chris Smeenk, who will hopefully write COI Story V on inflation someday soon. Richard Burian showed great patience in getting a manuscript of acceptable length from me. In the end, Paul Brinkman had to explain my own COIs to me. Finally, the paper owes much to the incisive criticism of an anonymous referee. *Dea mea invita, opus conficere non poteram.*

*which scientists in disparate fields, periods, and locales have used the explanatory power of their ideas as evidence. Scientists such as Copernicus, Kepler, Newton, Darwin, and Einstein all availed themselves of a pattern of reasoning that I have dubbed common-origin inferences (or COIs). A COI traces some striking coincidences back to a common origin (typically some causal structure or mechanism). This then provides an explanation for these coincidences, which is counted as evidence for the explanation.*

### Introduction

In history of science, it is commonplace to deny that there is such a thing as the scientific method. Historians are thus extremely reluctant to make any claims about scientific method that transcend the local contexts in which science is done. As a result, they overlook non-trivial elements of scientific methodology that are common to many traditions across disciplines, locales, and periods. By drawing attention to one such element, I argue for the importance of looking at non-local aspects of scientific methodology. The element that I focus on is a pattern of inductive<sup>1</sup> inference for which I have coined the term common-origin inference (*COI*). The danger with a project like this is that one ends up indiscriminately putting episodes in the history of science on the Procrustean bed of one's preconceived philosophical categories. This problem was endemic in the cottage industry in History and Philosophy of Science in the 1970s with its case studies of scientific change (see, e.g., Howson 1976). My *COI* project differs in two important respects from these earlier endeavors. First, *COIs* only capture one aspect of scientific methodology and are unlikely to be mistaken for a new model of scientific methodology as a whole. Second, I take history and not philosophy as my starting point. *COI's* claim to our attention comes from their ubiquitous use in scientific practice, past and present, not from an a-priori philosophical expectation that their usage should be ubiquitous.

### *COI* preliminaries: explanation and evidence

In choosing *COIs* as my example of a non-local aspect of scientific methodology, I take issue with the widely held view among philosophers of science that the explanatory power<sup>2</sup> of a theory does not count as evidence for that theory, i.e., that explanatory power should not affect a scientist's decision to accept or reject a theory. What makes *COIs* incompatible with this

1. I use this term to refer to *any* ampliative non-truth-preserving inference.

2. By explanatory power I mean the ability to provide answers to "why"-questions with their contrast classes fully specified so as to make them unambiguous (van Fraassen 1980, Ch. 5, Sec. 2.8).

philosophical position? *COIs* trace striking coincidences back to common origins. This then provides an *explanation* for these coincidences, which is counted as *evidence* for the explanation. *COIs* are thus a subspecies of what Gilbert Harman (1965) dubbed *Inference to the Best Explanation (IBE)*. In inductive practice, both in everyday life and in science, one routinely assigns evidentiary value to explanatory power. The dominant view in modern philosophy of science, however, appears to be that explanation and evidence are two completely separate things. Evidence is provided to justify claims *that* something is the case. Explanations are provided to answer questions *why* something is the case. Such questions, Bas van Fraassen (1980) and others have argued, depend so strongly on who is asking and in what context, that the answers only have value in that context and for those sharing the interests and presuppositions of the person raising the question. Explanations, their slogan goes, do not have epistemic but only pragmatic value. An important subsidiary goal of my paper is to show how strongly this view is at odds with scientific practice.

Here is a simple example to illustrate both the difference and the intimate connection between explanation and evidence. Consider the following pair of sentences:<sup>3</sup>

- (1) There are many people waiting at the bus stop *because* the bus is late.
- (2) The bus is late *because* there are many people waiting at the bus stop.

In sentence (1), the role of the word “because” is explanatory. The fact *that* there are many people waiting is not at issue. What the sentence offers is an explanation for *why* there are many people waiting. In sentence (2), the role of the word “because” is justificatory or evidentiary, not explanatory. That there are many people at the stop does not explain why the bus is late. The bus might be late because—i.e., explanatory because—the roads are slippery or because the driver overslept. Sentence (2) is not concerned with explaining *why* the bus is late. It describes the inductive inference from the observation that there are many people waiting at the stop to the assertion *that* the bus is late (for whatever reason).

What makes this a reliable inference? A Humean answer would be that the constant conjunction of “seeing many people at the stop” and “seeing the bus show up” shortly thereafter has produced a mental habit of expecting the latter whenever experiencing the former. This is not a very plausible answer. What makes the inference reliable is that the bus being late

3. The example was inspired by the discussion of different uses of the word “because” in Salmon (1995, pp. 16–18).

provides the most natural explanation for there being many people at the stop. Of course, the explanation may still be wrong. Inductive reasoning is not infallible. Maybe it is a holiday and you are not the only one who did not realize the bus is following a different schedule. Infinitely many other scenarios are possible. Yet, you confidently infer from seeing many people at the stop that the bus is late. Scientists make such inferences all the time. So do lawyers.<sup>4</sup> So does Sherlock Holmes. So does renowned artist David Hockney when he argues that artists in the early Renaissance used optical aids to achieve greater realism in their paintings.<sup>5</sup> This is what Harman called *Inference to the Best Explanation (IBE)*. Pragmatist Charles S. Peirce called it “abduction” (Van Fraassen 1980, p. 19; Lipton 2001b, p. 95).

4. In his classic textbook on evidence for law students, John H. Wigmore (1935) identifies this pattern of reasoning as the most common argument form in the court room: “the deductive form is rarely encountered in judicial trials. The inductive form is the normal one. . . . The evidence is offered in this form: . . . ‘Today, P, who was bitten yesterday by a dog, died in convulsions; therefore, probably the dog had hydrophobia.’ Now in [this example] the fact desired to be inferred (. . . the hydrophobia) is put forward as the best explanation (let us call it) of the evidential fact. But though it may be plausible or probable, yet the question must be faced, Are there not other explanations, possible or probable? E.g., . . . P’s convulsions may have been due to epilepsy or to strychnia” (Wigmore 1935, p. 54). Wigmore points out that judicial trials only involve four basic argumentative strategies: “(a) The first is the proponent’s *desired inference* from his supposed evidential fact. (b) Then the opponent may attempt to *explain away* the effect of this fact by pointing out some other and more plausible inference. Or (c) the opponent may deny that this supposed evidential fact is a fact. Or, finally, (d) the opponent, neither denying nor explaining away the first fact, may set up a *rival fact*” (Ibid., pp. 54–55). These dynamics are also encountered outside the courtroom. I am indebted to George Smith for providing these passages.

5. Hockney (2001) suggests that these artists used a concave mirror to project an image onto a canvas, then made a few marks to indicate all the important lines before finishing the painting from life. To argue his case, Hockney skillfully uses the explanatory power of the thesis itself. If it is true that the old masters used optics, this could explain a host of otherwise puzzling features in their paintings. To be sure, he presents other evidence, such as lists of the artists’ effects that include optical equipment, as well as contemporary documents with what could be construed as descriptions of the technique they allegedly used. Such documents, however, are few and far between. To explain this scarcity, Hockney assumes that the artists went to great lengths to hide their methods. Still, despite the paucity of contemporary evidence, Hockney has a strong case. His most convincing evidence is provided by his collaborator, the physicist Charles Falco. Especially effective is the use of multiple vanishing points in intricate table-cloth patterns and anomalies in the foreshortening of lutes, globes, and skulls that would result if lenses or mirrors had shifted ever so slightly while the artist was making his marks on the canvas with the projected image, and of various other kinds of subtle distortions that one would expect from the use of lenses and mirrors. Falco’s smoking gun is a table cloth painted out of focus in Lorenzo Lotto’s *Husband and Wife* dated 1523–1524 by the Hermitage where the painting is now located (Hockney 2001, pp. 60–61). Note that the case of Hockney and Falco hinges almost entirely on the evidentiary value of explanatory power.

Although it is well-entrenched in the modern philosophy of science literature, Harman's slogan is misleading. In an illuminating exchange on problems and prospects of *IBE* as a pattern of scientific inference, Peter Lipton (2001a, b) and Wes Salmon (2001a, b) agree that it would be more accurate to use the "less memorable phrase, 'inference to the best of the available competing explanations, when the best one is sufficiently good'" (Lipton 2001b, p. 104, quoted approvingly in rebuttal by Salmon [2001b, p. 132]). I will use the phrase "Inference to the Best Explanation" for any move from the explanatory power of a claim or a theory to a certain type and level of commitment to that claim or theory. Lipton uses the phrase in a similar fashion: "the interesting idea is simply that we sometimes decide how likely a hypothesis is to be correct in part by considering how good [or, in Lipton's standard terminology, *how lovely*] an explanation it would provide, if it were correct" (Lipton 2001b, p. 93, my emphasis). He goes on to say that he wants to promote "the idea of explanatory considerations providing a guide to inference" (Ibid.). Needless to say, Lipton is my cup of tea.

Returning now to my bus-stop example, we see that sentences (1) and (2) are not so wholly unconnected after all. The explanatory sentence (1) states the explanation that is key to the evidentiary relationship expressed in sentence (2). Similar connections between explanatory and evidentiary statements can be found in many examples of both everyday and scientific reasoning. The reader can easily produce his or her own examples of atomic sentences *p* and *q* such that the sentences "*p* because *q*" and "*q* because *p*" stand in the same relation to one another as sentences (1) and (2) above.

Thus, explanation and evidence cannot be as neatly separated as many modern philosophers of science would have it. The discrepancy between philosophy of science and scientific practice on this score is remarkable. A glance at the table of contents of some popular philosophy of science anthologies (Boyd et al. 1991; Salmon, et al. 1992; Curd and Cover 1998)<sup>6</sup> shows that there is far less overlap than one would expect between the literature on scientific evidence (under the rubric of "confirmation theory") and the literature on scientific explanation.<sup>7</sup> It is not that philosophers have failed to recognize the common practice of using explanatory power as evidence. The trouble is that many philosophers have disputed the evidentiary value of explanatory power and thereby the reliability of *IBEs*.

6. Lipton (1995) is an exception. It devotes more space to the intersection of the literatures on confirmation and explanation.

7. On scientific explanation see, e.g., Kitcher et al. (1985), Kitcher and Salmon (1989), Knowles (1990), Lipton (1991, 1996).

I can discern two different motivations—neither of them justified in my opinion—that are behind this attitude. The first motivation is related to the abuse of *IBE* in the cause of scientific realism. Bas van Fraassen has been the most vocal critic of such abuse (for discussion see Okasha 2000). In his book on *IBE*, Lipton (1991, Ch. 9) convincingly shows that the *IBE* from the empirical successes of science to scientific realism makes for an extremely weak argument. The failure of this particular *IBE*, however, hardly discredits *IBE* in general.

Philosophers, like van Fraassen, who endorse the idea that explanatory power does not have epistemic but only pragmatic value typically assign epistemic value exclusively to empirical adequacy.<sup>8</sup> In scientific practice, however, explanatory power and empirical adequacy both have epistemic value, and the latter does not automatically trump the former.<sup>9</sup>

One might object that it does not matter whether we label explanatory considerations pragmatic or epistemic, as long as we acknowledge that they play a role in scientific practice. Van Fraassen, for one, certainly does not want to deny this.<sup>10</sup> The problem is that the slogan “explanation is pragmatic” is typically taken to imply that an explanation for one person need not be an explanation for another. Whether an explanation is good or bad, on this view, is in the eye of the beholder. Evidence is usually not taken to be pragmatic in the sense of being relative to the scientist making the call. Reserving the honorific title “epistemic value” for empirical

8. In van Fraassen's case, we have to look no further than the blurb on the back of *The Scientific Image* (1980): “the explanatory power of a theory is a pragmatic aspect which goes beyond its empirical import, but which provides no additional reasons for believing it.”

9. My favorite example of the scales tipping in favor of explanatory power is general relativity. The best evidence for the basic idea of general relativity—roughly: gravity is space-time curvature—is that it explains the equality of inertial and gravitational mass, which in Newton's theory is just a coincidence. In the absence of this explanatory feat, one would have to be extremely gullible to believe in the correctness of the field equations of general relativity just because they happen to predict the correct value for the anomalous advance of the perihelion of Mercury.

Special relativity provides a historically more clear-cut example. The first published reaction to Einstein's 1905 paper introducing special relativity came from Walter Kaufmann who presented data on the velocity dependence of electron mass which he claimed ruled out special relativity and fit well with two alternative theories (Miller 1998, 2001, p. 216). Einstein defended special relativity essentially by arguing that these competing theories were seriously lacking in explanatory power. He wrote: “Abraham's and Bucherer's theories of the motion of the electron yield curves that are significantly closer to the observed curve than the curve obtained from the theory of relativity. However, the probability that their theories are correct is rather small, in my opinion, because their basic assumptions concerning . . . the moving electron are not suggested by theoretical systems that encompass larger complexes of phenomena” (Einstein 1907, sec. 10).

10. For a concise statement of his views, see, e.g., Van Fraassen (1985, pp. 280–281).

adequacy and labeling explanatory power and other virtues of a theory “pragmatic” suggests a hierarchy of considerations going into theory acceptance that is not borne out by scientific practice.

A different motivation for keeping evidence and explanation separate can be found among philosophers of science whose main interest is confirmation theory. They want to avoid the concept of explanation as much as possible, arguing that the problem of analyzing and articulating the evidentiary relation between data and theories is difficult enough without getting mixed up in the even bigger problem of clarifying explanatory relations at the same time.<sup>11</sup> Such bracketing of the problem of explanation while working on the problem of confirmation may make sense, if one sees philosophy of science primarily as a normative enterprise, in which one tries to formulate guidelines that scientists ought to abide by regardless of whether or not they do so in practice. If, however, one sees philosophy of science primarily as a descriptive enterprise, as I do, and one is interested primarily in elucidating scientific practice, as I am, then one cannot afford to postpone dealing with the problem of explanation and one had better come to terms right away with the evidentiary value that scientists routinely assign to explanatory power and consequently with *IBEs*.

Convincing accounts of what makes for good explanations may be even harder to come by than convincing accounts of what makes for good evidence. Given *COI*'s historical track record, however, and given what philosophy of science has taught us about explanation so far, I am optimistic that the kind of explanation involved in *COIs* will pass muster with whatever satisfactory account(s) of explanation philosophy of science will hopefully produce some day.

One important bonus of accepting that explanatory power has evidentiary value is that it offers a cheap way out of another notorious incongruity between philosophy of science and scientific practice. According to the Duhem-Quine under-determination thesis there is always an infinite number of theories compatible with any finite number of data. In principle or “*sub specie aeterni*” [sic]—as Einstein (1954, p. 236), borrowing Spinoza's phrase, said about Henri Poincaré's geometrical conventionalism—the Duhem-Quine thesis is correct, as is Poincaré's conventionalism.

11. Lipton (2001, p. 100) takes issue with the closely related charge (Salmon 2001a, p. 68) that *IBE* cannot be (part of) a satisfactory account of scientific inference in the absence of a satisfactory account of scientific explanation. In his reply, Salmon fully concedes Lipton's point: “we must have some strong intuitions about the nature of explanation even to begin constructing an account of explanation . . . As Carnap pointed out . . . unless we engage in preliminary clarification of the explicandum, we may miss the very concept we are trying to explicate” (Salmon 2001b, p. 122).



In practice, however, a scientist never faces the embarrassment of riches suggested by the Duhem-Quine thesis. The scientist's problem is typically to find just *one* satisfactory theory. The key to understanding this striking contrast between armchair philosophy and scientific practice is to note that an empirically adequate theoretical concoction does not a satisfactory theory make. In particular, the vast majority of the infinitely many alternative "theories" suffer from debilitating explanatory deficiencies. Since explanatory power is a virtue on a par with empirical adequacy in scientific practice, these alternative "theories" will simply not be taken into consideration. In *The Value of Science* (1905), Poincaré, in effect, endorses this cheap "explanatory deficiency"-escape from the Duhem-Quine conundrum (see below).

### *COI* project

I present a number of *COIs* in the history of science from different periods, locales, and disciplines. *COIs* are exceedingly common in science as well as in everyday life, and I am hardly the first to draw attention to them. *COIs* bear a close resemblance to two of Newton's famous "rules of reasoning" at the beginning of Book III of the *Principia*. And William Whewell's "consilience of inductions" can be seen as stringing together various *COIs* to form a meta-*COI* or *MCOI*.<sup>12</sup> My own interest in what I have come to call *COIs* was triggered by Wes Salmon's reconstruction of the case for molecular reality in the early years of the twentieth century as a common-cause argument (Salmon 1984, pp. 213–221).<sup>13</sup>

A *COI*, like an *IBE*, can be defined as an inference from a statement of the form "If it were the case that *X*, then that would explain observations/phenomena *a, b, c, . . .*" to "It is, in fact, the case that *X*." The distinguishing characteristic of a *COI* is that *X*—a statement, a model, or an idea, no matter how well or how poorly articulated—wields its explanatory power by tracing a number of otherwise puzzling coincidences (the "*a, b, c, . . .*" mentioned above) to a common origin. The everyday example of an *IBE* that I gave above is a *COI*. It traces what, on the face of it, is an odd coincidence (many different people waiting for the bus) to a common origin (the bus being late).

12. Pronounced "McCoy" as in Dr. Bones McCoy of *Star Trek* or Jack McCoy of *Law and Order*. These TV shows—like *The Practice* or *The X Files*—are themselves rich sources for examples of *IBEs*, *COIs*, and *MCOIs*.

13. Salmon drew on earlier work by Mary Jo Nye (1972). Following Nye, Salmon took a special interest in the argument for molecular reality made by Jean Perrin in his popular 1913 book *Les Atomes* (reprinted in translation as Perrin 1990). Salmon took the term common-cause argument from his teacher Hans Reichenbach (1971, pp. 157–167).



*COIs* thus involve what I will call *common-origin explanations*. Such explanations can be seen as answers to questions of the form “Is it just a brute fact that  $a, b, c, \dots$  or is there some common origin from which  $a, b, c, \dots$  spring?” In a scientific context, common-origin explanations may be anything from an embryonic idea to a mature theory, though we shall see that in the typical and most interesting cases it is something closer to the former. In fact, for the *COIs* examined in this paper, we shall see that while they provide exceptionally strong warrant for the conclusion that the phenomena they tie together are due to the same structure or mechanism, the support provided for more specific proposals for what that structure or mechanism might be is much weaker.<sup>14</sup> *COIs* thus function more as important constraints on adequate explanations and thus as inferences to a class of explanations (which typically will yet have to be fully articulated) than as inferences to the best of a set of available full-blown explanations. This does not mean that it is a mistake to think of *COIs* as *IBEs*. Recall that *IBE* is the slogan; the operative notion is that explanatory considerations have evidentiary value. And the main *COIs* examined in this paper have provided some of the strongest evidence ever produced by science on how to cut nature at the joints.

To appreciate the role and the force of *COIs*, it is important to recognize that theories serve (at least) two different purposes in scientific practice. They provide representations of selected features of reality and they provide instruments for investigating such features.<sup>15</sup> Given the debates over the semantic versus the syntactic view of theories and over realism versus anti-realism, there has been a tendency in modern philosophy of science to focus on the first of these two aspects. The key question has been: which (if any) features of the models of a theory represent features of reality? Or, in terms of the syntactic rather than the semantic view: which (if any) terms in the axiomatization of a theory are genuinely referential?

As we shall see, theories launched by *COIs* tend to serve the purpose of representation by providing taxonomies for the relevant field of study. *COIs* seek to establish “natural kinds”—or “phenomena kinds” as Laura Snyder has called them—for the classification of a range of phenomena. The theory should also provide some means of marshalling evidence for the proposed taxonomy, so that its proponents can obtain some assurance

14. The claim, for instance, that there has to be *some* common-origin explanation for why there are many people at the stop is better supported than the more specific common-origin explanation that the bus is late. Other common-origin explanations are also possible.

15. The relevant distinction was suggested to me by George Smith. It is loosely based on the distinction between theory<sub>1</sub> and theory<sub>2</sub> in Bromberger (1992, pp. 56–57).

that they are not being led down a garden path.<sup>16</sup> It is mainly for this reason that good *COIs* should at least provisionally identify some mechanism connecting the phenomena it groups together. A *COI* will be particularly fruitful if this identification is specific enough to develop theoretical instruments to work out the details of the taxonomy it suggests. If scientists find that more and more phenomena fall into place when ordered with the help of these instruments, their confidence will grow that the *COI* has picked out genuine phenomena kinds. If, however, more and more phenomena resist the kind of grouping suggested by the *COI*, the scientists may eventually conclude that they have been led down a garden path.

The representations and the instruments provided by a theory launched by a *COI* call for different types of commitment on the part of a scientist. As far as representation is concerned, the scientist needs a certain degree of belief in the ontological claim that the taxonomy suggested by the *COI* corresponds to genuine phenomena kinds. As far as the instruments are concerned, the scientist only needs a certain degree of belief in the effectiveness or fruitfulness of the theory as an aid to further investigation of the phenomena under consideration. The latter type of commitment will be particularly strong in what I shall call the “context of pursuit.”<sup>17</sup> This is where scientists decide which research project to work on. Given that both available time and available resources are finite, these are crucially important decisions. Scientists need to feel that the project they decide to pursue promises greater rewards than alternative projects.

The ontological commitment to the basic taxonomy of a theory will be much stronger in what I shall call, following a suggestion by Kari Gwen Coleman, the “context of persuasion.”<sup>18</sup> This is where scientists try to con-

16. The terminology “guarding against garden paths” is George Smith’s, who has analyzed this function of theories in great detail in the case of Newton’s gravitational theory (Smith 2002a, b) and in the case of J. J. Thomson’s work on the electron (Smith 2001b).

17. The term was suggested to me by Allan Franklin. Both the concept and the term can also be found in Laudan (1981, p. 182), and in McKinney (1995).

18. I prefer the terms “pursuit” and “persuasion” over Hans Reichenbach’s terms “discovery” and “justification,” which have become too loaded. For recent ruminations about Reichenbach’s distinction, see Schickore and Steinle (2002). The brief discussion below benefited from comments by Geoffrey Hellman.

An innocuous interpretation of Reichenbach’s distinction is as the distinction between reasoning going into finding some claim and reasoning purporting to establish that claim. Failing to distinguish the two is then simply to run afoul of the genetic fallacy. Reichenbach, however, did not introduce the distinction for such a trivial purpose. He maintained that logico-philosophical analysis should be restricted to the context of justification and that the context of discovery should be left to historians and psychologists. Since Kuhn, Feyerabend, and Laudan (1981, Ch. 11), it has become commonplace to note that many “external” elements, which for Reichenbach made the context of discovery

vince the relevant community—be it their peers, (prospective) research students or employers, funding agencies, the general public, etc.—of the significance and validity of their results, a task that can be especially daunting if those results at the time (no matter how important they may later turn out to be) are both still open to severe criticism and sharply at odds with received wisdom. As we shall see, scientists, in their attempts to persuade their colleagues of the merit of an idea, will often turn to the same *COIs* that convinced them to pursue the idea to begin with.

It is important that a *COI* at least provisionally identify some structure or mechanism that can be held responsible for the connection between the phenomena it ties together.<sup>19</sup> I thus suspect that most interesting *COIs* will be *CCIs*, *Common-Cause Inferences*. I want to leave open the possibility, however, that *CCIs* do not exhaust the class of *COIs*.

What is the relation between *COIs* and *CCIs* and what Salmon calls common-cause arguments in the context of his general account of explanation and causation (Salmon 1984)?<sup>20</sup> Salmon's so-called "ontic conception of explanation" provides a natural framework for the analysis of *COIs* and *CCIs*: "The ontic conception sees explanations as exhibitions of the ways in which what is to be explained fits into natural patterns or regularities. This view . . . usually takes the patterns and regularities to be causal" (Salmon, 1985a, p. 293). This seems to be an accurate characterization of common-origin explanations. I probably differ with Salmon over how good the fit into natural patterns and regularities has to be for a common-origin explanation to be convincing. I suspect that Salmon would have wanted such fits to be governed by well-developed quantitative theo-

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off-limits for logico-philosophical analysis, also infect the context of justification. What is not often noted is that the converse is also true. The same arguments and considerations used in the context of justification often played a crucial role in the context of discovery as well. (The role of the Michelson-Morley experiment in the context of justification of the special theory of relativity is a famous exception to this rule [Stachel 1982].) If one is going to attempt a logico-philosophical analysis of these considerations at all, it is therefore only natural to do so in both contexts.

19. An example of the dangers of using *COIs* that fail to identify such a mechanism is provided by Antoine Lavoisier's argument for identifying what he originally called "eminently respirable air" as the gas of oxygen, the acidifying principle (Levere 2001, p. 64). Lavoisier's argument for this claim can be seen as an instance of Mill's method of agreement. Lavoisier noticed that burning phosphorus and sulphur and dissolving the resulting gases in water produced acids. He likewise found that the acids formed from carbon and nitrogen contained eminently respirable air. Lavoisier famously and erroneously drew the conclusion that oxygen was the long sought-after acidifying principle. He did not propose any mechanism for how eminently respirable air causes acidity. This example illustrates that *COI* is not the same as Mill's method of agreement.

20. I am grateful to Chris Smeenk for helping me clarify this relation.

ries. I am particularly interested in cases where the fit is governed by no more than an embryonic idea.

Salmon's conception of causation is too narrow for my purposes. Under my definition, *CCIs* can be of two kinds. The "common cause" can either be some event or substance with causal efficacy or some causal structure, network, or mechanism. Following Reichenbach's (1971, pp. 157–167) discussion of the common-cause principle, Salmon tends to focus on the former. In scientific practice, however, the latter is much more common.

A closely related problem with Salmon's focus on events and substances is that it reinforces one of the unfortunate side-effects of the ubiquitous use of *CCIs* in scientific practice, namely the tendency to reify causal structures as substances with causal efficacy. John Stachel (1994, p. 149) has introduced the phrase "the fetishism of mathematics" for this type of sin.

For instance, in *COI STORY III*, about the disagreement between relativists (Einstein et al.) and ether theorists (Lorentz et al.) over the interpretation of Lorentz invariance, the *COI* on the side of the relativists is to Minkowski space-time. This *COI* would hardly be acceptable as an argument to settle the dispute over the interpretation of Lorentz invariance, if it required its proponents to subscribe to a substantival rather than a relational ontology of space and time. Fortunately, as I have argued elsewhere (Balashov and Janssen forthcoming), the force of this particular *COI* is independent of one's ontology of space and time. I think this is true in general: one cannot draw conclusions about the ontological status of theoretical entities simply from the fact that the existence of these entities is being inferred to via a *COI* or a *CCI*.

Salmon presented his common-cause argument for molecular reality in the context of the debate about scientific realism.<sup>21</sup> Any scientific realist who (tacitly) assumes that a strong *CCI* establishes that the common cause it points to must automatically be a causally efficacious substance had better keep in mind Virgil's famous line as he or she reads the Salmon-inspired *COI* stories in this paper: *Timeo Danaos et dona ferentis*.<sup>22</sup>

21. As it says in the blurb on the back of Salmon (1984): "Professor Salmon's theory furnishes a robust argument for scientific realism akin to the argument that convinced 20th-century physical scientists of the existence of atoms and molecules." For the record, I personally consider myself a Kantian or structural realist. I am indebted to Otávio Bueno for showing me that the latter is just the latest incarnation of the former.

22. Jon Dorling (1979) used this quotation as the motto for a paper in which he showed that some features of scientific methodology captured by Lakatos' methodology of scientific research programs (Lakatos 1970) are straightforward consequences of Bayes' theorem in confirmation theory. In this sense, the motto may in the end be turned against me: Wes Salmon (2001a, b) has argued that it should be possible to capture whatever features of scientific inference are captured by Lipton-style *IBE* in terms of Bayes' theorem.

### COI questions.

The important role of COIs in various key episodes in the history of science raises a number of interesting historical, historiographical, and philosophical questions. Although I do not shy away from suggesting some answers, the main goal of this paper is simply to draw attention to the questions. This is meant as a programmatic paper and I shall consider it a success if I can convince the reader that COIs represent a non-local element of scientific methodology worthy of further attention of historians and philosophers of science. The historical/historiographical questions I have in mind include:

1. Exactly what role do COIs play in different historical cases?
2. How many good COI stories can one find in the history of science?<sup>23</sup> What about COI stories where the inferred common origin

23. Plate tectonics, for instance, provides a picture-perfect example of a COI that exhibits many of the characteristics of the examples that I will discuss in this paper. Another example in geology is provided by the asteroid-impact theory of dinosaur-extinction. An excellent account of this episode is given in Powell (1998), which clearly shows how important COIs were in getting the impact theory accepted, even though Powell is a self-confessed Popperian (Powell 1998, Ch. 4). I am grateful to Tony Duncan for drawing my attention to this book.

The hottest COI story of contemporary science has got to be the one on inflation. The promise of the basic idea of an exponential expansion of the early universe to explain some striking coincidences left unexplained by the standard big bang model was used with great rhetorical skill by Alan Guth (1981) as evidence for inflation. Inflation, as he later put it, “looked too good to be wrong” (Guth 1997, p. 199). Or—to use Lipton’s phrase—the explanations provided by inflation are too lovely to be wrong. Many scientists have plunged into inflation although to date neither Guth nor anyone else has been able to produce a workable mechanism for the proposed rapid expansion. Meanwhile, however, measurements of the cosmic microwave background and findings by supernova hunters indicating that the expansion of the universe may be accelerating have provided evidence of a more traditional nature that Guth and company are on the right track. For historical and philosophical analysis of this episode, see Smeenk forthcoming.

Steadman (2001) provides an interesting COI story in art history. With the cunning use of COIs, he makes an unassailable case that Johannes Vermeer used a camera obscura. He considers, for instance, six different Vermeers, of which he has established earlier that they were done in the same room, and determines the points from which the scenes in these six paintings are viewed. He then reconstructs the rectangles enclosing the images obtained if we were to project these six scenes onto the back wall of the room with one and the same lens placed in the relevant viewpoints. The result is astonishing: “*In all six cases . . . this rectangle is almost exactly the same size as the relevant painting . . .*” How are we to interpret this extraordinary geometrical coincidence? (It can hardly be due to chance.) It can be very simply explained by the assumption that Vermeer was using a camera” (Steadman 2001, p. 102). Steadman considers the possibility that Vermeer used a mathematical perspective method but rejects this alternative as explanatorily deficient: “I can think of no plausible explanation as to why any mathematical perspective method should produce [the extraordinary

turned out to be bogus?<sup>24</sup>

3. What did the historical actors relying on *COIs* think justifies them? Can we discern general patterns if we compare the justification of *COIs* in different historical cases? For instance, is the justification in the context of pursuit different from the justification in the context of persuasion?<sup>25</sup>
4. Can the recognition of the role of *COIs* in different disciplines, periods, and locales be used to reverse the trend in history of science towards ever greater historicizing and contextualizing of the methods of scientific inquiry?
5. How, if at all, does the recognition of the role of a *COI* in a particular episode affect the way in which we write the historical narrative for that episode?
6. We shall see that *COIs* can provide a powerful engine for theory change. In those cases, explanatory deficiencies of the old theory rather than Kuhnian empirical anomalies drive theory change. How prevalent is this non-Kuhnian pattern?
7. What exactly is the relation between *COIs* (and *MCOIs*) and such well-known methodological prescriptions as Newton's "rules of reasoning," Whewell's "colligation of facts" and "consilience of inductions," Mill's "methods of experimental inquiry," and Peirce's "abductions" or, to use the modern term, "retroductions"?<sup>26</sup>

The more philosophical questions include:

1. What is the force behind *COIs*? Are they just an extension of a pattern of reasoning that, for all its reliability in everyday experience, may still fail when used to explore strange new worlds? Is there one

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geometrical coincidence mentioned above] so straightforwardly and simply accounted for by a camera technique" (Ibid., p. 155). I am grateful to Alan Shapiro for giving me these passages.

24. Some particularly instructive examples that immediately come to mind are Georg Stahl's phlogiston theory, late-nineteenth century ether theory, and William Paley's natural theology (Gliboff 2000). These examples are by no means embarrassments for my *COI* project. Many of the taxonomic claims made by these theories survived their demise: think of the unification of combustion, respiration, and calcination in the case of the phlogiston theory or the unification of electricity, magnetism and optics in the case of the ether theory. The explanatory power of these theories also translated into the development of effective theoretical tools for the further exploration of the relevant phenomena. It is no condemnation of these theories that these tools eventually became obsolete.

25. Had Jane Austen known about this distinction, she would surely have called the sequel to *Sense and Sensibility* and *Pride and Prejudice*, *Pursuit and Persuasion*.

26. Good places to start looking for answers to this question are McMullin (2001) and Snyder (1997a).

answer to the first question or are there many different answers depending on what use is being made of a *COI*?

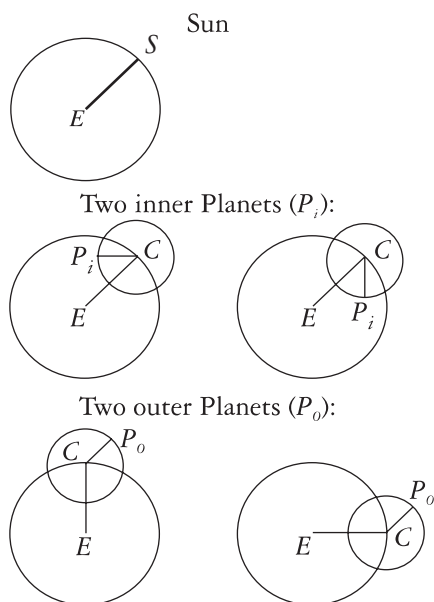
2. Is mere unification enough for a convincing *COI* or is it crucial that the inferred common origin be some sort of causal structure? In other words, are *COIs*, as the examples examined in this paper suggest, better understood in the spirit of the Salmon causal account of explanation than in the spirit of the Friedman-Kitcher unification account, or is this an overhasty generalization?
3. Can we endorse *COIs* in science while at the same time rejecting them as a way of arguing for philosophical positions (think of the controversial use of *IBE* in arguing for scientific realism)?
4. Can we formulate general criteria to distinguish good *COIs* from bad *COIs*? In particular, can we do so independently of their ultimate success or failure?
5. Does the analysis of *COIs* help us locate the flaws in philosophical arguments to the effect that explanations only have pragmatic value, a conclusion so strongly at odds with scientific practice that I strongly suspect it to be false?
6. Does the analysis of *COIs* yield normative prescriptions for current scientific practice—or even criteria for funding decisions, a virtue Lakatos famously claimed for his methodology—or is it merely a descriptive tool?

### ***COI* Story I: Copernicus, Kepler, and Poincaré and the Rotation of the Earth**

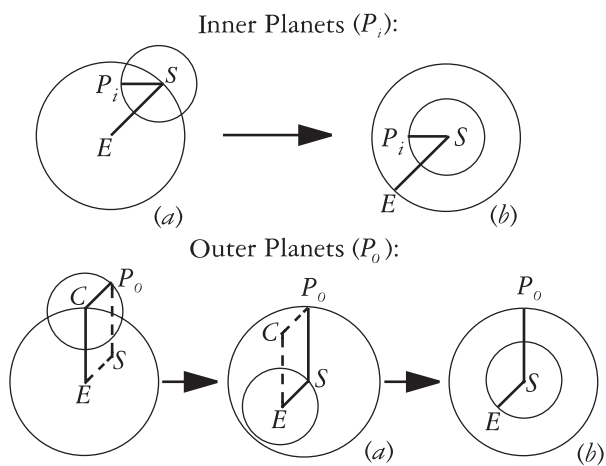
Consider the simplified Ptolemaic models for the motion of the sun and the planets in Fig. 1.<sup>27</sup> The motion of the sun, *S*, is represented by a uniform circular motion around the Earth, *E*. This motion, like all motions in Fig. 1–3, is counterclockwise. To account for retrograde motion, two uniform circular motions are ascribed to each planet. The planet, *P*, is moving on a circle (the epicycle) the center of which, *C*, is moving on a larger circle (the deferent) with the Earth, *E*, at its center. It was well-known among Ptolemaic astronomers that the deferent motions of Mercury and Venus, the two inner planets, and the epicycle motions of Mars, Jupiter, and Saturn, the three outer planets, are correlated with the motion of the mean sun. In the simple models in Fig. 1 this can be seen as follows. For inner planets *EC* always remains parallel to *ES*. For outer planets *CP*, always remains parallel to *ES*.

27. Figures 1–3 are adapted from Figs. 8 and 9 in Swerdlow and Neugebauer (1984, Vol. 2, pp. 570–571, for the actual figures; Vol. 1, pp. 56–57, for discussion of these figures).





**Figure 1.** Simplified Ptolomaic deferent-epicycle models (all for the same time).



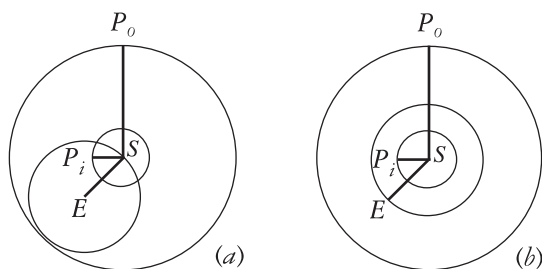
**Figure 2.** Changing the frame of reference: from geocentric (a) to heliocentric (b).

It was also well-known that the models of a planet moving around the Earth could be replaced by models that have the planet moving around the sun and that have either the sun moving around the Earth or the Earth moving around the sun. For the simple models in Fig. 1, the transition to these alternative models is illustrated in Fig. 2, for inner planets in the top half of the figure, for outer planets in the bottom half. For an inner planet,  $P_i$ , one obtains the model with the planet going around the sun rather than around the Earth by taking  $C$  (in Fig. 1) as the actual position of the sun rather than as just giving the direction  $ES$  of the sun. Inspection of the line segments  $ES$  and  $SP_i$  (in Fig. 2) shows that the two models in the upper half of Fig. 2 produce the same relative motion of the Earth, the sun, and the inner planet. In both models  $P_i$  goes around  $S$ , but in model (a)  $S$  goes around  $E$ , while in model (b)  $E$  goes around  $S$ .

To make the same transition for an outer planet one intermediate step is required. The point  $S$  needs to be added to the points  $E$ ,  $P_o$ , and  $C$  to form the parallelogram  $ESP_oC$ . As its label suggests, the point  $S$  is taken as the position of the sun. Inspection of  $ESP_oC$  shows that the three models in the lower half of Fig. 2 produce the same relative motion of the Earth, the sun, and the outer planet. In the model on the left,  $P_o$  goes around  $C$  and  $C$  goes around  $E$ . In model (a) in the middle  $P_o$  goes around  $S$  and  $S$  goes around  $E$ . In model (b) on the right both  $P_o$  and  $E$  go around  $S$ .

Before Nicholas Copernicus' *De Revolutionibus* (1543), the alternative models illustrated in Fig. 2 were not accorded any physical significance. Johannes Regiomontanus, for instance, to whose work Copernicus owed much of his understanding of Claudius Ptolemy's *Almagest*, presented alternate models for individual planets one of which would have the sun moving around the Earth and one of which would have the Earth moving around the sun (Swerdlow and Neugebauer 1984, pp. 56–57). Copernicus was the first to replace the original Ptolemaic models for all planets by models that have the Earth moving around the sun (cf. the models (b) for  $P_i$  and  $P_o$  in Fig. 2). In terms of the simplified models of Figs. 1–2, he set the radii  $EC$  of the deferents of the inner planets  $P_i$  and the radii  $CP_o$  of the epicycles of the outer planets  $P_o$  equal to the radius  $SE$  of the sun's orbit around the Earth, combined the separate models for the motion of the sun and the motion of the various planets into one unified model, and thus arrived at the heliocentric model, a simplified version of which (with two rather than five planets) is shown under (b) in Fig. 3. Note that the two models in Fig. 3 produce the same relative motions: the line segments connecting  $E$ ,  $S$ ,  $P_i$ , and  $P_o$  are the same in diagrams (a) and (b).

One of the main virtues of the model, as Copernicus himself realized, is that it offers an explanation for what in the Ptolemaic theory is just an unexplained coincidence, namely the striking correlation between one of the



**Figure 3.** Changing the physics: (a) Tycho Brahe's geocentric model; (b) Copernicus's heliocentric model.

two main components of the motion of every planet with the motion of the sun. Copernicus could explain these correlations by tracing them to a common origin. One component in the motion of every planet as seen from Earth reflects the Earth's own motion around the sun. This particular argument for Copernicus' heliocentric model thus has the form of a *COI*, from correlated motions left unexplained by Ptolemy to a moving Earth. One can quibble over the nature of the common origin in this case. As we shall see, both for Copernicus and Kepler it was a common cause.<sup>28</sup>

The *COI* actually does not quite get us all the way to the notion of a moving Earth. Its conclusion is rather that all planets orbit the sun. It remains an open question whether the Earth orbits the sun or the other way around. The former alternative corresponds to Copernicus' heliocentric model of 1543, the latter to Tycho Brahe's geocentric model of 1588 (see model (a) in Fig. 3). Tycho's model also explains the correlated motions left unexplained by the Ptolemaic models. In Tycho's model, they reflect the sun's motion around the Earth.

The *COI* from correlated motions to "all planets orbit the sun" thus provides a strong argument for preferring Copernicus' model or Tycho's

28. The correlations between the motions of the planets and the motion of the mean sun are a direct kinematical consequence of the motion of the earth postulated by Copernicus, but they are not dynamically caused by the earth's motion. At least one late-sixteenth/early-seventeenth century astronomer found Copernicus' explanation of these correlations unsatisfactory precisely because it failed to provide such a dynamical explanation. In his astronomy textbook, *In sphaeram Ioannis de Sacrobosco*, which went through sixteen printings between 1570 and 1618 (Lattis 1994, p. 44), Christoph Clavius wrote: "*Ptolemaeus per epicyclum reddit omnium apparentiarum causam in Sole*" ("Ptolemy using the epicycle attributes the cause of all appearances [e.g., retrograde motion, maximal elongation] to the sun." Lattis 1994, pp. 131–132). From a *COI*-ish point of view, Clavius' Ptolemaic explanation is unsatisfactory because it fails to identify any mechanism through which the sun could orchestrate the synchronized motion of the planets.

over Ptolemy's, but it does not provide a basis for deciding between Copernicus and Tycho. Other arguments are needed for that.

Historically, all three of these astronomical models were embedded in Aristotelian physics. The Copernican model contradicts the most basic tenets of this Aristotelian framework. How can the Earth, made out of the four terrestrial elements and thereby destined to sit motionless at the center of the sub-lunar sphere, circle around a celestial body, made out of the weightless fifth element? How can it spin on its own axis without leaving the clouds and the birds behind? What about the vast empty space one has to assume between Saturn and the fixed stars to account for the absence of any measurable stellar parallax? What about offending religious sensibilities by turning the Earth into the third rock from the sun?<sup>29</sup> Tycho's model clearly is much more conservative, even though it has its share of problems when taken seriously as a physical model. The Crystalline spheres thought to carry the planets in the Aristotelian theory intersect one another in Tycho's model. Another distinctly non-Aristotelian feature is that the planets circle another celestial body rather than the center of the universe as their Aristotelian nature would require them to do.

With hindsight and freed from Aristotelian prejudices, one might say that Copernicus' model is the natural implementation of the insight provided by our *COI*, whereas Tycho's model is mainly an attempt to prevent the insight from wreaking havoc on Aristotelian physics and Church dogma. Such notions as "the natural implementation," however, are context-dependent, and my motivation for exploring *COIs* in this paper is to pinpoint an element of scientific methodology that transcends local contexts. That our *COI* cannot decide between Copernicus and Tycho goes to show that there is more to scientific reasoning than *COIs*, some of it inextricably tied to period, locale, and discipline. Comparison of the relative merits of the models of Copernicus and Tycho, however, does suggest the following general rule. If a *COI* can be used in support of competing explanations, damage control is not a good guide to decide between the available options.

This *COI* story illustrates some other general features of *COIs*. The use of *COIs* suggests an alternative to the Kuhnian mechanism of theory change through the accumulation of empirical anomalies and the proliferation of different articulations of the reigning paradigm to come to terms with those anomalies (see, e.g., Kuhn 1957, pp. 139–141, for an account

29. This was offensive not because it banned the central drama of creation to a theater off-Broadway, but because, as the astronomer Clavius put it, "quite rightly has Nature placed the earth at the center of the cosmos, since such a worthless and crude body ought to be uniformly separated from all parts of the heavens, which is a body of highest excellence" (Lattis 1994, p. 120).

along these lines of the background to Copernicus' innovation). The alternative mechanism involving *COIs* exploits explanatory deficiencies of the old theory instead. It does so by turning unexplained coincidences in the old theory into input for *COIs* to its successor theory. Finding such coincidences, of course, is no trivial task. A coincidence may not even have been recognized as standing in need of an explanation before some tentative but plausible explanation has been put forward. In this particular case, the need for an explanation of at least part of the correlation between the motion of the inner planets and the motion of the sun, a phenomenon known as maximal elongation,<sup>30</sup> had been felt even in medieval times and several astronomers had made the inference from maximal elongation to the idea that Mercury and Venus are satellites of the sun (see, e.g., Eastwood and Graßhoff 2000).

Another feature of *COIs* is that they can overcome weighty objections. In this case, the *COI* turned out to be strong enough to overcome the objection that the Copernican model flies in the face of both Aristotelian physics and Catholic doctrine and the objection that it does not have a plausible explanation for why the stellar parallax it predicts could not be detected.

An important feature that I have not emphasized so far is that *COIs* often involve what cosmologist Michael Turner has dubbed "forward-engagement." Copernicus' model, in a sense, is nothing but a promissory note. As it stands, it is too preliminary and faces too many problems to be a viable alternative to the Ptolemaic-Aristotelian theory. It could only fully replace that theory after it had been elaborated and augmented by the work of Galileo, Kepler, and Newton. Copernicus' work is thus what Thomas Kuhn has called "revolution-making" as opposed to "revolutionary" (Kuhn 1957, p. 135). I much prefer the term "forward-engaged."<sup>31</sup> Copernicus' work is forward-engaged in that it takes an advance on the new physics it calls for.

How close are Copernicus' own pronouncements to the language of *COI*? In chapter 10 of Book I of *De Revolutionibus*, Copernicus himself actually makes a statement strikingly similar to our *COI*: "So we find . . . an admirable symmetry in the Universe, and a clear bond of harmony in the motion and magnitude of the Spheres such as can be discovered in no other wise. For here we may observe why the progression and retrogression

30. As can be seen in Fig. 2, an inner planet can never get further away from the sun *S* than the radius of its epicycle.

31. Thrice-divorced Allan Franklin, however, has objected that it sounds too matrimonial.

appear greater for Jupiter than Saturn, and less than for Mars, but again greater for Venus than for Mercury . . . *All these phenomena follow from the same cause, namely Earth's motion*" (quoted and annotated in Kuhn 1957, p. 180, my emphasis).

A little earlier in this same chapter, Copernicus had stated the metaphysical principle that can serve as the justification for the rule of inference used in this passage: "We thus rather follow Nature, who producing nothing vain or superfluous often prefers to endow one cause with many effects" (quoted in Kuhn 1957, p. 179).

Kuhn does not attach great importance to the passages in which Copernicus describes his own argument like a *COI*. Kuhn focuses on the first sentence of the quotation above:

Throughout [Book I, Chapter 10] Copernicus's emphasis is upon the "admirable symmetry" and the "clear bond of harmony in the motion and magnitude of the Spheres" . . . The sum of the evidence drawn from harmony is nothing if not impressive.

But it may well be nothing. "Harmony" seems a strange basis on which to argue for the earth's motion, particularly since the harmony is so obscured by the complex multitude of circles that make up the full Copernican system. Copernicus' arguments . . . appeal, if at all, . . . to [the practising astronomer's] aesthetic sense and to that alone. They had no appeal to laymen, who . . . were unwilling to substitute minor celestial harmonies for major terrestrial discord (Kuhn 1957, p. 181).

Kuhn's assessment of the strength of Copernicus' argument and, consequently, of the importance of Book I compared to the rest of *De Revolutionibus* is thus radically different from mine. He writes: "[I]t is [the later books of *De Revolutionibus*] which by showing for the first time that the astronomer's job could be done, and done more harmoniously, from a moving earth, provided a stable basis from which to launch a new astronomical tradition. Had Copernicus' cosmological First Book appeared alone, the Copernican Revolution would and should be known by someone else's name" (Kuhn 1957, p. 184).

I do not know whether Kuhn's assessment is shared by many modern Copernicus scholars. For my purposes this does not matter. I only want to use Kuhn here as an example of how the recognition of the role of *COIs* in historical episodes can make a difference in how, as a historian, one evaluates such episodes.

For Kuhn the considerations described by Copernicus in the passage quoted above can only be understood in the context of the Neoplatonic

tradition in which Copernicus worked and which sanctioned his otherwise unconvincing appeals to simplicity and harmony. For Kuhn the arguments of Book I “could and did appeal primarily to that limited and *perhaps irrational* subgroup of mathematical astronomers [with a] Neoplatonic ear for mathematical harmonies” (Kuhn 1957, p. 181, my emphasis).

Contrast this assessment with the one suggested by my *COI* story. First, casting Copernicus’ argument as a *COI* rather than as an appeal to simplicity and harmony frees the rationality of Copernicus’ reasoning from its Neoplatonic context. Second, Book I is the most important part of *De Revolutionibus* in my *COI* story. This is where Copernicus provides a strong argument for pursuing the heliocentric model even in the face of what looked like insurmountable difficulties. As Kuhn concedes, the later books neither make the astronomer’s job any easier nor its results more accurate. Here Copernicus is still very much a Ptolemaic astronomer, albeit one preferring an unusual coordinate system. Positional astronomy took a quantum leap only when astronomers began to dare questioning Ptolemy’s data—Copernicus, the old quip goes, was still saving Ptolemy rather than the phenomena—and when Kepler introduced elliptical orbits. Although Copernicus could eliminate the deferent or the largest epicycle for every planet, he still had to introduce scores of minor epicycles to match the accuracy of the Ptolemaic models. As a result, his full-blown model looks every bit as monstrous as the models he set out to replace.

This brings us to another issue. In the preface of his book, Copernicus famously compared the result of the accumulated efforts of his predecessors to a “monster rather than a man:” “as though an artist were to gather the hands, feet, head and other members for his images from diverse models, each part excellently drawn but not related to a single body” (quoted in Kuhn 1957, p. 139). Copernicus’ work was initially motivated, it seems, by these considerations—epitomized by his almost visceral dislike of that most objectionable of devices: the equant—not by the explanatory deficiencies of the Ptolemaic theory at the heart of my *COI* story. When did Copernicus hit upon his *COI* and what role did it play in the further development of his ideas? George Joachim Rheticus’ *Narratio Prima* (1540) sheds some light on these questions. In a section entitled “The Principal Reasons Why We Must Abandon the Hypotheses of the Ancient Astronomers,” Rheticus lists six such reasons, both aesthetic and *COI*-ish ones. The most interesting one for our purposes is the fifth since it traces Copernicus’ metaphysical justification for trusting his *COI* to Galen’s *De usu partium* (Rosen 1971, p. 137, notes 118 and 119). Rheticus writes:



Fifthly, mathematicians as well as physicians must agree with the statements emphasized by Galen here and there: “Nature does nothing without purpose” and “So wise is our Maker that each of his works has not one use, but two or three or often more.” Since we see that this one motion of the earth satisfies an almost infinite number of appearances, should we not attribute to God, the creator of nature, that skill which we observe in the common makers of clocks? For they carefully avoid inserting in the mechanism any superfluous wheel or any whose function could be served better by another with a slight change of position. What could dissuade my teacher, as a mathematician, from adopting a serviceable theory of the motion of the terrestrial globe, when he saw that . . . this hypothesis . . . sufficed, for the construction of a sound science of celestial phenomena . . . this one motion alone seems to be the cause of every apparent inequality at a distance from the sun, in the case of the three superior planets, and . . . in the case of Venus and Mercury (Rosen 1971, p. 137).

Copernicus’ *COI* is featured prominently in Johannes Kepler’s *Mysterium Cosmographicum* (1596), a book notorious for Kepler’s speculations about the relation between the five known planets and the five regular solids. One could argue that these speculations show that Kepler was too trigger-happy when it came to *COIs* and that he saw common origins even in cases where coincidences were just that—coincidences. However, Kepler was extraordinarily careful and level-headed in his scientific methodology (see also McMullin 2001, p. 285; and Wilson 1972).

What convinced Kepler of Copernicus’ model? He writes:

My confidence was first established by the magnificent agreement of everything that is observed in the heavens with Copernicus’s theories. . . . However what is far more important is that, for the things at which from others we learn to wonder, only Copernicus magnificently gives the explanation,<sup>32</sup> and removes the cause of wonder, which is not knowing causes. . . . For instance, they do not give the reasons for the number, extent, and time of the retrogressions, and why they agree precisely, as they do, with the positions and mean motion of the Sun. On all these points, as a magnificent order is shown by Copernicus, the cause must necessarily be found in it (Duncan 1989, pp. 75, 77).

Kepler recognizes that Copernicus’ *COI* cannot be used to decide between the models of Copernicus and Tycho:

32. The Latin has “rationem” and is thus not quite as suggestive of *IBE*.

Copernicus's postulates<sup>33</sup> cannot be false, when so reliable an explanation of the appearances—an explanation unknown to the ancients—is given by them, *insofar as it is given by them*. This was seen by [Tycho Brahe,] who although he entirely disagreed with Copernicus on the position of the Earth, yet retained from him the point which gives us the reasons for matters hitherto not understood, that is, that the Sun is the center of the five planets. For the proposition that the Sun is motionless at the center is a more restricted intermediate premise for the derivation of retrogressions, and the general proposition that the Sun is in the center of the five planets is sufficient (Duncan 1989, p. 77, my emphasis).

To justify his preference for Copernicus over Tycho, Kepler appeals to the argument that one mechanism for both the Earth and the planets is better than two, one for the sun and one for the planets: “Yet for Copernicus’s . . . setting the Sun at the center of the universe, and the Earth in motion round it, there were other reasons. For, to turn from astronomy to physics or cosmography, these hypotheses of Copernicus not only do not offend against the Nature of things, but do much more to assist her. She loves simplicity, she loves unity. Nothing ever exists in her which is useless or superfluous, but more often she uses one cause for many effects” (Duncan 1989, p. 77). The last two sentences of this passage describe the same metaphysical justification for relying on *COIs* that we found in Book I of *De Revolutionibus*.

These passages from *Mysterium Cosmographicum* clearly illustrate the importance of *COIs* in the context of pursuit. Explanatory power was of paramount importance in Kepler’s decision to throw in his scientific fate with Copernicus’ model. What seems to have convinced Galileo Galilei, by contrast, were the observations made with his own telescopes. Readers suspicious of ascribing evidentiary value to explanatory power may be tempted to use the example of Galileo to argue that *COIs* belong only in the context of pursuit. They only play a role, one might argue, to help scientists decide what to work on in the absence of “hard” evidence (such as the data from Galileo’s telescope). Before accepting the theory that a scientist has thus decided to work on, the argument would continue, the promissory notes of explanatory gain had better pay some dividend in terms of empirical data that fit well with the scientist’s own theory and are sufficiently problematic for the competition.

33. The Latin has “principia” but the translation has the singular “postulate.” The Latin “ratio” is once again rendered as “explanation” (cf. note 32).

Even if one accepts this argument (which I do not), explanatory considerations would still carry epistemic and not just pragmatic value. It is not some idiosyncrasy on Kepler's part to make decisions about how to allocate his valuable research time and finite resources by comparing the explanatory promise held by the available options. Implicit in the passage from Kepler quoted above is the notion that explanatory value *ought* to count.

In fact, although explanatory power does not seem to have been instrumental in convincing Galileo of Copernicus' model originally, he did recognize its evidentiary value, and used it in the context of persuasion. During "The Third Day" of his *Dialogue Concerning the Two Chief World Systems* (1632), Galileo has his spokesperson Salviati explain how retrograde motion provides a strong argument for ascribing an annual motion to the earth: "[W]hat are we to say of the apparent movement of a planet, so uneven that it not only goes fast at one time and slow at another, but sometimes stops entirely and even goes backward a long way after doing so? To save these appearances, Ptolemy introduces vast epicycles, adapting them one by one to each planet . . . all of which can be done away with by one simple motion of the earth" (Galileo 2001, p. 397).

The fair-minded Sagredo then asks Salviati to explain how retrograde motion comes about in the Copernican system. Salviati obliges: "Sagredo, you will see them come about in such a way that the theory of this alone ought to be enough to gain assent for the rest of the doctrine from anyone who is neither stubborn nor unteachable" (Ibid., p. 398). The force of this argument for Galileo derives from the same metaphysical principles cited by Copernicus and Kepler. Sagredo lists as "commonly adopted by all philosophers:" "Nature does not multiply things unnecessarily; . . . she makes use of the easiest and simplest means for producing her effects; . . . she does nothing in vain, and the like" (Galileo 2001, p. 460).<sup>34</sup> The first of these three principles is a restatement of Ockham's razor: *non sunt multiplicanda entia praeter necessitatem* (entities are not to be multiplied beyond necessity).

That explanatory power continues to count as evidence long after the dust has settled is illustrated by Poincaré's *The Value of Science* (1905). Poincaré addresses a misunderstanding of one of the conventionalist claims in his *Science and Hypothesis* (1903), namely that the "two proposi-

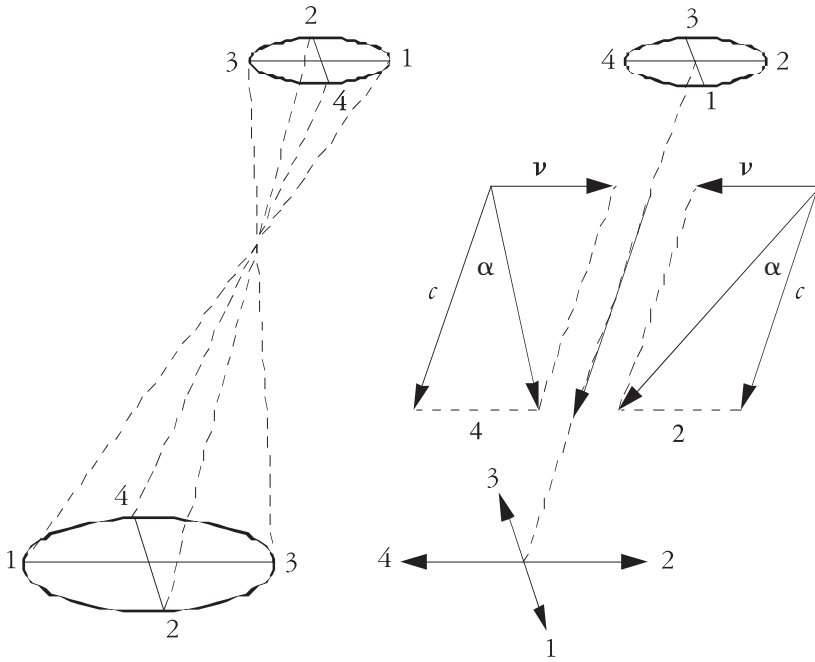
34. The original Italian reads: "Comunemente ricevuti da tutti i filosofi, come che la natura non moltiplica le cose senza necessità, e che ella si serve de' mezzi più facili e semplici nel produrre i suoi effetti, e che ella non fa niente indarno, ed altri simili" (Galilei 1970, p. 470).

tions, the earth turns round, and, it is more convenient to suppose that the earth turns round, have one and the same meaning" (Poincaré 2001, p. 350). Poincaré had been shocked to find that some readers took this to mean that the choice between Copernicus and Ptolemy is merely a matter of convention. Given the emphasis in modern philosophy of science on the Duhem-Quine thesis and the under-determination of theories by evidence, a philosopher can hardly be blamed for reading Poincaré's pronouncement in this manner. And she would hardly be comforted by Poincaré's first line of defense: "This truth, the earth turns round, was put on the same footing as Euclid's postulate, for example. Was that to reject it?" (Ibid.). She might well answer Poincaré's rhetorical question affirmatively! After all, for Poincaré Euclidean geometry was a convention—one so convenient that it would never be abandoned, but a convention nonetheless.<sup>35</sup>

The key to understanding why the underdetermination problem has never been much of an issue in scientific practice is that the Duhem-Quine thesis only takes into account one criterion for theory choice, namely empirical adequacy. Once we factor in other criteria, such as explanatory power, the underdetermination problem all but disappears. And whatever may turn out to be wrong with a theory inferred to via a strong *COI*, such a theory will be able to boast of considerable explanatory power. Poincaré accordingly offers a full-blown *COI* as his most powerful and effective response to his readers' misconstrual of his conventionalist pronouncements. A similar response will work for Duhem-Quine underdetermination charges leveled against any theory inferred to via (and/or backed by) a strong *COI*. After giving a *COI* for the Earth's rotation on its own axis (which takes as input the apparent daily rotation of the stars and various manifestations of the Coriolis force), Poincaré writes:

[W]hat shall we say of its revolution around the sun? Here again, we have three phenomena which for the Ptolemaist are absolutely independent and which for the Copernican are referred back to the same origin; they are the apparent displacements of the planets on the celestial sphere [see Figs. 1–3], the aberration of the fixed stars, the parallax of these same stars. Is it by chance that all the planets admit an inequality whose period is a year and that this period is precisely equal to that of aberration, precisely equal besides to that of parallax? To adopt Ptolemy's system is to answer yes; to adopt that of Copernicus is to answer no; this is to affirm that there is a bond between the three phenomena (Poincaré 2001, pp. 351–352).

35. Only a decade later, Einstein's general theory of relativity would prove Poincaré's prediction wrong.



**Figure 4.** Stellar parallax and stellar aberration.

Poincaré's argument is just a beefed-up version of Copernicus' *COL*. The extra strength comes from an increase in the number of coincidences traced to the common origin (two extra planets, for instance, had meanwhile been discovered: Uranus in 1781 and Neptune in 1846) but also from a diversification of the input (aberration and parallax).

Consider the evidence provided by the phenomena of aberration and parallax. These two phenomena are illustrated in Fig. 4, parallax on the left, aberration on the right.<sup>36</sup> Because of the Earth's rotation around the sun, the apparent position of a star changes over the course of a year. The numbers 1 through 4 in the diagram on the left in Fig. 4 label the position of the Earth at three-month intervals and the corresponding apparent positions of the star. The effects of parallax are proportional to the ratio of the diameter of the Earth's orbit and the distance of the star. Even for the closest stars this ratio is so small that the effect was not detected until the late 1830s (by Friedrich Bessel). The effects of aberration, proportional to

36. This figure was taken from Janssen and Stachel (forthcoming).

the ratio of the Earth's orbital velocity and the velocity of light, are more pronounced and were first detected by James Bradley in the late 1720s. Whereas parallax is due to changes in the Earth's position over the course of a year, aberration is due to changes in the Earth's velocity. An astronomer trying to aim a telescope at a star must compensate for the Earth's velocity, just as people walking through the rain must tilt their umbrellas in the direction in which they are moving if they want to keep their feet dry. The numbers 1 through 4 in the diagram on the right in Fig. 4 label the velocity of the Earth at three-month intervals and the corresponding apparent positions of the star. (The orbit of the Earth can be represented by a point in this diagram since parallax produces a much smaller effect than aberration.) Notice that the sequence of apparent motions on the right in Fig. 4 lags behind the sequence on the left by about three months. This immediately told Bradley that the effect he had observed could not be parallax as he had originally thought. Both effects, however, provide strong evidence that the Earth moves around the sun rather than the other way around. Poincaré's beefed-up *COI* thus does eliminate both Ptolemy's and Tycho's model.

In the standard hypothetico-deductive account of confirmation, the observations of Bradley and Bessel confirm the heliocentric hypothesis because they confirm predictions derived from the hypothesis. But the confirmation can also be construed as *IBE*-type confirmation. As Poincaré notes, the observations can be made compatible with the Ptolemaic model by adding two more circular motions with a period of one year to the set of motions already ascribed to the sphere of the fixed stars. The resulting theory, while empirically adequate, would nonetheless be unsatisfactory, because it fails to explain the odd coincidence that these motions, like the motions of the planets, are so strongly correlated to the motion of the Sun. From this perspective, the observations of aberration and parallax are thus seen as reinforcing Copernicus' *COI*.

More importantly for my purposes, the quotation from Poincaré illustrates that *COIs* can play a role in the context of persuasion as well as in the context of pursuit. And it also illustrates how *COIs* can cure cases of the Duhem-Quine disease.

#### ***COI Story II: Newton's "Rules of Reasoning," Whewell's "Consilience of Inductions," and Darwin's "One Long Argument"***

Both for Copernicus and for Kepler the reliability of *COIs* ultimately derived from a metaphysical principle. Copernicus formulated it as: "Nature, who producing nothing vain or superfluous often prefers to endow one cause with many effects" (Kuhn 1957, p. 179); Kepler as: "[Nature] loves simplicity, she loves unity. Nothing ever exists in her which is use-

less or superfluous, but more often she uses one cause for many effects” (Duncan 1989, p. 77). Both formulations remind one of Isaac Newton’s phrase—in the Motte-Cajori translation of the *Principia*—“Nature is pleased with simplicity and affects not the pomp of superfluous causes” (Cajori 1934, Vol. 2, p. 398).

The phrase occurs in the section entitled “Rules of Reasoning in Philosophy”<sup>37</sup> inserted at the beginning of Book III in the second edition of the *Principia*. The first two rules are:

Rule 1. *No more causes of natural things should be admitted than are both true and sufficient to explain their phenomena.* As the philosophers say: Nature does nothing in vain, and more causes are in vain when fewer suffice. For nature is simple and does not indulge in the luxury of superfluous causes.

Rule 2. *Therefore, the causes assigned to natural effects of the same kind must be, so far as possible, the same.* Examples are the cause of respiration in man and beast, or of the falling of stones in Europe and America, or of the light of a kitchen fire and the sun, or of the reflection of light on our earth and the planets (Cohen and Whitman 1999, pp. 794–795).

Newton’s phrase “as the philosophers say” suggests that the metaphysical principle that Copernicus, Kepler, and Galileo subscribed to was fairly common at the time. According to Rheticus, Copernicus got it from Galen. Newton is probably borrowing from Aristotle and Ockham. In the original Latin, the passage “more causes are in vain when fewer suffice” reads: *frustra fit per plura quod fieri potest per pauciora*. This is a verbatim quotation of an alternative version of Ockham’s razor.<sup>38</sup>

Newton only invoked rules 1 and 2 four times,<sup>39</sup> all four instances

37. This is the Motte-Cajori translation of “Regulae Philosophandi.” It is rendered as “Rules for the Study of Natural Philosophy” in the authoritative new translation of the *Principia* by I. B. Cohen and Anne Whitman (1999, p. 794).

38. In his edition of the *Principia*, Volkmar Schüller (1999, pp. 648–649, annotation for p. 380) identifies various passages in Aristotle that could be the source for Newton’s “Nature does nothing in vain” (“Natura nihil agit frustra”). Schüller suggests that Newton took this passage and the formulation of Ockham’s razor from *De Motu Libri X* by Franciscus Bonamicus (Florence, 1591). We cannot be sure, however, that Newton read Bonamicus. Mamiani (2002) suggests that Newton adapted all four rules of reasoning from a set of similar rules in his *Treatise on the Apocalypse* (ca. 1672) and that he took these rules from Robert Sanderson’s *Logicae Artis Compendium*. Newton purchased a copy of the third edition of this book, which had come out in 1631, in 1661 (Mamiani 2002, p. 388). It seems much more plausible, however, that Newton got the first two rules from Aristotle and Ockham, as Schüller suggests, either through Bonamicus or through another source.

39. George Smith, private communication.



occurring at the beginning of Book 3 and in the context of two closely related arguments. These arguments, however, are central to Newton's overall argument for universal gravitation.<sup>40</sup> First, in proposition 4, Newton rehearses his famous "moon test" and concludes: "And therefore that force by which the moon is kept in its orbit, in descending from the moon's orbit to the surface of the earth, comes out equal to the force of gravity here on earth, and so (by rules 1 and 2) is that very force which we generally call gravity" (Cohen and Whitman 1999, p. 804). He makes essentially the same point in the scholium to this proposition (Ibid., p. 805). Newton's argument can easily be recast in *COI* language.

Newton invokes rules 1 and 2 again in proposition 5 which states that gravity is also what keeps both the planets in orbit around the sun and the satellites of Jupiter and Saturn in orbit around these planets. Newton explains that these phenomena are "of the same kind as the revolution of the moon about the earth, and therefore (by rule 2) depend on causes of the same kind, especially since it has been proved that the forces on which those revolutions depend are directed towards the centers of Jupiter, Saturn, and the sun, and decrease according to the same ratio and law (in receding from Jupiter, Saturn, and the sun) as the force of gravity (in receding from the earth)" (Ibid., p. 806). In the scholium to proposition 5, he writes: "Hitherto we have called 'centripetal' that force by which celestial bodies are kept in their orbits. It is now established that this force is gravity, and therefore we shall call it gravity from now on. For the cause of the centripetal force by which the moon is kept in its orbit ought to be extended to all the planets, by rules 1 [and] 2" (Ibid.).

Under William Whewell's interpretation, Newton's rules of reasoning are closely related to *COIs*.<sup>41</sup> His interpretation is interesting in its own right, but is also important for understanding Charles Darwin's "one long argument" (Darwin 1958, p. 435) in *On the Origin of Species*.<sup>42</sup> Before he was willing to reveal his theory of evolution through natural selection to the world, Darwin wanted to make sure that his exposition of the theory met the highest standards of scientific reasoning. In Victorian England,

40. Note that these arguments belong squarely in the context of persuasion. Newton's own path to universal gravitation was considerably more complicated. See Wilson (1970, 1989) for a detailed account of the development of Newton's ideas from the 1660s to the publication of the *Principia* in 1687.

41. One Newton scholar who without much reservation accepts Whewell's reading as expressing Newton's own intent is Bill Harper. See, e.g., Harper (1991), where Whewell is mentioned in the very first paragraph. In Harper (2002), Whewell is no longer mentioned explicitly, but as far as I can tell, Harper's Whewellian interpretation of Newton's rules of reasoning is still the same.

42. I am grateful to John Beatty, Michael Reidy, and, especially, Rich Bellon, Paul Brinkman, and Laura Snyder for advice on Darwin and Whewell.

that meant he had to aspire to become the Newton of natural history. The Newton of the Victorians, when it came to scientific methodology, was the Newton of Whewell. Darwin was thoroughly familiar with the work of both Whewell and John Herschel.<sup>43</sup> It should therefore come as no surprise that his pronouncements on methodology sound remarkably Whewellian.<sup>44</sup>

Ironically, Whewell, rather than hailing Darwin's work as a brilliant application of his own ideas, never accepted Darwin's theory. The mighty *MCOI*—or “Consilience of Inductions” to use Whewell's term—that Darwin put forward in support of his theory was not strong enough to overcome the Reverend's religious scruples.<sup>45</sup> The story goes that Whewell kept the *Origin* off the shelves of the library of Trinity College of which he was headmaster (Ruse 1975, p. 180). Herschel followed suit, famously denouncing natural selection as “the law of hickledy pickledy” (Ibid.).<sup>46</sup>

43. Darwin read Herschel (1830) early in 1831 during his last year as a student in Cambridge. If we are to trust Darwin's later recollections, Herschel's influence was rivaled only by Alexander von Humboldt's. In his autobiography, Darwin wrote that these two authors stirred in him a “burning zeal to add even the most humble contribution to the noble structure of natural science” (Ruse 1975, p. 164; Hull 1973, p. 116; Browne 1995, p. 133). When Darwin writes about “one of our greatest philosophers” in the first paragraph of the *Origin*, he is writing about Herschel. It is unclear whether Darwin ever read Whewell's *The Philosophy of the Inductive Sciences*. He did read several other books by Whewell, however, and the two men knew each other personally. Darwin also read with great interest Herschel's detailed review of Whewell's *Philosophy*. In an entry “Books to be read” in one of his notebooks, Darwin jotted down: “—From Herschel's Review . . . I see I MUST STUDY Whewell on Philosophy of Science” (Ruse 1975, p. 164).

44. There is an extensive literature on Darwin's debt to such nineteenth century philosophers as Whewell and Herschel. Some of the better-known sources are: Ellegård (1957), Ghiselin (1969), Hull (1973), and Ruse (1975). Much of this literature is dated by its reliance on hypothetico-deductivism. Although much of his book remains useful, Ghiselin's basic position, for instance, has become untenable: “Darwin applied, rigorously and consistently, the modern hypothetico-deductive scientific method” (Ghiselin 1969, p. 4). There seems to be general agreement that John Stuart Mill is not that important for understanding Darwin. Thomas Henry Huxley, “Darwin's bulldog,” was very much enamored of Mill (Ellegård 1957, sec. 6). Darwin approved of Huxley's endorsement of Mill—in part no doubt because of Mill's positive verdict of the *Origin* (see note 46)—but personally he was drawn more to Herschel and Whewell.

45. In her book on Whewell and Mill currently in the works, Laura Snyder will offer a more charitable interpretation of Whewell's reaction to Darwin.

46. Mill's reaction was kinder. In his 1862 edition of his *System of Logic*, he paid Darwin a compliment, albeit a somewhat back-handed one, for the *Origin*: “It is unreasonable to accuse Mr. Darwin (as has been done) of violating the rules of induction. The rules of induction are concerned with the condition of proof. Mr. Darwin has never pretended that his doctrine was proved. He was not bound by the rules of induction, but by those of hypothesis. And these last have seldom been more completely fulfilled. He has opened a path of inquiry full of promise, the results of which none can foresee” (Ellegård 1957, p. 375;

In examining Newton's rules of reasoning, Whewell considers and rejects various interpretations of the first rule, then settles on an interpretation that in effect makes it a precursor to his own "Consilience of Inductions." Whewell writes:

[T]he Rule so understood is so far from being unmeaning or frivolous, that it expresses one of the most important tests which can be given of a sound physical theory. It is true, the explanation of one set of facts may be of the same nature as the explanation of the other class: but then, that the cause explains *both* classes, gives it a very different claim upon our attention and assent from that which it would have if it explained one class only. The very circumstance that the two explanations coincide, is a most weighty presumption in their favour. It is the testimony of two witnesses in behalf of the hypothesis; and in proportion as these two witnesses are separate and independent, the conviction produced by their agreement is more and more complete.<sup>47</sup> When the explanation of two kinds of phenomena, distinct, and not apparently connected, leads us to the same cause, such a coincidence does give a reality to the cause, which it has not while it merely accounts for those appearances which suggested the supposition. This coincidence of propositions inferred from separate classes of facts, is exactly what we noticed in the *Novum Organon Renovatum* (b. ii. c. 5, sect. 3), as one of the most decisive characteristics of a true theory under the name of *Consilience of Inductions* (Butts 1968, p. 330).

The *Novum Organon Renovatum*—Whewell's update of Francis Bacon's *Novum Organon*—is the second part of the third edition of his *Philosophy of the Inductive Sciences Founded upon their History*.<sup>48</sup> In Book 2, Ch. 5, sec. 3, the passage referred to above, we find:

We have here spoken of the prediction of facts *of the same kind* as those from which our rule was collected. But the evidence in favour

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Browne 2002, p. 186). In my terms, Mill acknowledges the force of Darwin's argument in the context of pursuit, but not in the context of persuasion.

47. In one of his contributions to the famous debate with Mill (see Snyder 1997a for an insightful discussion of this debate), Whewell similarly compared the case of "striking and surprising coincidence" giving a theory "a stamp of truth beyond the power of ingenuity to counterfeit," to "a case of interpreting an unknown character, in which two different inscriptions, deciphered by different persons, had given the same alphabet. We should, in such a case, believe with great confidence that the alphabet was the true one" (Butts, p. 295). The example of independent witnesses was also used by Wes Salmon in his reconstruction of Perin's argument for molecular reality (Salmon 1984, pp. 220–221).

48. The first edition was published in 1840. Between the years 1858 and 1860, the third edition appeared in three installments (Butts 1968, p. 3).

of our induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false supposition could after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and un contemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from that being the point where truth resides.

Accordingly the cases in which inductions from classes of facts altogether different have thus *jumped together*, belong only to the best established theories which the history of science contains. And as I shall have occasion to refer to this peculiar feature in their evidence, I will take the liberty of describing it by a particular phrase; and will term it the *Consilience of Inductions*.

It is exemplified principally in some of the greatest discoveries. Thus it was found by Newton that the doctrine of the Attraction of the Sun varying according to the Inverse Square of this distance, which explained Kepler's *Third Law* . . . explained also his *First* and *Second Laws* . . . ; although no connexion of these laws had been visible before. . . . Here was a most striking and surprising coincidence, which gave to the theory a stamp of truth beyond the power of ingenuity to counterfeit<sup>49</sup> (Butts 1968, p. 153).

Whewell's "Consilience of Inductions"<sup>50</sup> is thus what I call a meta-*COI* or *MCOI*. A *MCOI* strings together a number of *COIs* to similar common origins to form a more powerful inference to a deeper common origin. The passages from Book 3 of the *Principia* quoted above can be used to illustrate how a *MCOI* works.

The first *COI* starts from the observation that the ratio of the acceleration of falling bodies at the surface of the earth and the moon's centripetal acceleration is equal to the inverse square of the ratio of the earth's radius and the radius of the lunar orbit. On the basis of this remarkable coincidence, Newton mounts a *COI* to the claim that the earth attracts other bodies with an inverse square law. The acceleration of Jupiter's moons and the acceleration of the planets in their orbits around the sun likewise sug-

49. The last clause ("which . . . counterfeit") can also be found in one of Whewell's responses to Mill (see note 47).

50. For an illuminating diagram illustrating "Consilience of Inductions," see Losee (2001), p. 113.

gests that Jupiter and the sun attract other bodies with an inverse square law. Newton can thus mount two more *COIs*, one for Jupiter, one for the sun. The three separate *COIs* can then be combined to form one *MCOI* to the more general claim that *all* bodies attract other bodies with an inverse square law.

It may seem that one could also reconstruct this argument as one ordinary *COI* to the more general claim with a more diversified list of coincidences as input, i.e., a list pertaining to falling bodies, the moon, Jupiter's satellites, the planets, etc. (cf. Poincaré's reinforcement of Copernicus' *COI* in *COI* Story I). The problem with such a reconstruction is that it threatens to make the input of the *COI*, the unexplained coincidences, dependent on its output, the common-origin explanation. Darwin's argument for evolution through natural selection provides a good example of why that would be a problem.<sup>51</sup>

It is a striking coincidence,<sup>52</sup> crying out for an explanation, that the fabled Galapagos finches all look like finches from the mainland with their beaks carefully tailored to their specific niche in the ecosystem. This coincidence can be used as fodder for a *COI*. It is also a striking coincidence that the flipper of a seal has the same bone structure as the wing of a bat. This can be exploited for another *COI*. In the absence of Darwin's theory, however, it would be stretching the notion of "a striking coincidence" to claim that it is a striking coincidence that both *A* and *B* happen to be true, where *A* stands for "flippers and wings share the same bone structure" and *B* stands for "beaks are perfectly adapted to their use." The combination of the two *COIs*, one from *A*, the other from *B*, is best conceived, not as one *COI* with the more diversified input (*A*, *B*), but as a *MCOI* stringing together the *COIs* from *A* and *B* separately. It provides strong evidence for a theory if it can trace widely different "classes of facts"—to use a phrase that both Whewell and Darwin use—to a common origin. In our example there are two (or three) such classes, one with facts about adaptation (and maybe a separate class with facts about the geographical distribution of species) and one with facts about homologies (i.e., non-functional structural similarities). Given these considerations, Whewell's "Consiliences of Induction" are best understood as *MCOIs*. *COIs* themselves are akin to what Whewell calls "colligations of facts" (cf. Snyder 1997b, sec. 3).

51. I am grateful to Alan Richardson for alerting me to this problem.

52. A striking coincidence is one that cries out for a common-origin explanation even if there is no such explanation in the offing. To determine whether a coincidence brought to our attention by a particular common-origin explanation is "striking," one should ask: does the coincidence still call for a common-origin explanation if the original explanation turns out to be wrong?

Whewell offers a new justification for relying on *MCOIs* (and, by implication, *COIs*). Where Newton, Kepler, and Copernicus all appealed to a metaphysical principle to the effect that Nature is simple and economical, Whewell appeals to the track record of these inferences. Immediately following the quoted passage from the chapter on Newton in *The Philosophy of the Inductive Sciences*, he writes: "That Newton's First Rule of Philosophizing . . . authorizes the inferences which he himself made, is really the ground on which they are so firmly believed by philosophers" (Butts 1968, p. 330).

Whewell's confidence in *MCOIs* is even stronger than Kepler's confidence in *COIs*. According to Kepler, "Copernicus's postulates cannot be false, when so reliable an explanation . . . is given by them, *insofar as it is given by them*" (Duncan 1989, p. 77, my emphasis). In the quotation from *Novum Organon Renovatum* above, there is no counterpart to the italicized caveat in Kepler's formulation. Whewell writes: "No false supposition could after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and un contemplated" (Butts 1968, p. 153). He put it more bluntly on other occasions: "I believe the history of science offers no example in which a theory supported by such consiliences, had been afterwards proved to be false" (Ibid., p. 295).

Whewell has been ridiculed for these bold claims. As van Fraassen, skeptical of anything related to *IBE*, comments: "The historical claim [Whewell] made was that no truly consilient theory has ever later turned out to be false. Newtonian mechanics [rather, Newton's theory of universal gravitation] was, of course, his great and paradigmatic example of such a truly consilient theory (!)" (van Fraassen 1985, p. 267). Here van Fraassen has a point. To heighten the sense of irony one could add Whewell's other favorite example, the wave theory of light. The natural response on behalf of Whewell, I want to argue, is to add Kepler's caveat to Whewell's cavalier pronouncements. Neither Copernicus' *COI* to a moving earth, nor (Whewell's version of) Newton's *MCOI* to universal gravitation quite establishes what their authors believed. The former only allows us to infer to the claim that any viable theory dealing with the solar system must make the same structure responsible for the correlations that formed the input of Copernicus' *COI*. Given Tycho's alternative structure, Copernicus' *COI* does not license the claim that the Earth moves around the sun. Many other features of Copernicus' model, such as its circular orbits, do not receive any support from Copernicus' *COI* either. The same is true *mutatis mutandis* for (Whewell's version of) Newton's *MCOI* to universal gravitation. It establishes that any viable theory of gravity must make one structure responsible for the falling of stones, the tides, the motion of the moon, the motion of the planets, the motion of Jupiter's satellites, and

all other phenomena tied together by the *MCOI*. As impressive as this list is, it does not add up to establishing the Newtonian concept of universal gravitation (i.e., an attraction governed by an inverse square law between all masses in the universe all moving according to the laws of Newtonian mechanics in Newton's absolute space and time). Einstein's general theory of relativity attributes all these phenomena and more to a different structure, curved space-time, a structure that will no doubt itself be replaced one day in some version of quantum gravity.

Whewell nevertheless makes an important point. In *COI* speak: the claims inferred to in the (*M*)*COIs* of Copernicus, Newton, Darwin, and Einstein will continue to be true to the extent—and this is Kepler's caveat—that these (*M*)*COIs* licensed these claims in the first place. Thus, after Copernicus, we shall never again seriously consider an astronomical theory that treats the retrograde motion of the planets on a case-by-case basis. Copernicus' *COI* establishes once and for all that nature is not differentiated like that. After Newton, we shall never return to a theory of gravity that treats the terrestrial and celestial realms differently. Newton's *MCOI* establishes once and for all that nature is not like that. Similar claims can be made on behalf of many other *COIs*. As a final example, consider the following *COI* in James Clerk Maxwell's<sup>53</sup> "On the Physical Lines of Force" (1862): "The velocity of transverse undulations in our hypothetical medium . . . agrees so exactly with the velocity of light . . . that we can scarcely avoid the inference that *light consists of the transverse undulations of the same medium which is the cause of electric and magnetic phenomena*" (quoted and discussed in Harman 1998, p. 108). After this *COI*, no theory about electromagnetism that does not also cover light shall ever again be entertained seriously. The importance of Maxwell's result is not diminished by the fact that the medium mentioned so prominently in this passage was eventually relegated to the scrap heap of history. Van Fraassen is right to question whether any theoretical structure that is only tenuously connected to empirical data should be taken to represent reality, but the *COIs* listed above establish their legitimate conclusions beyond the shadow of a doubt.

*COIs* and *MCOIs* identify what Snyder has called "phenomena kinds." They provide a way of cutting nature at the joints. The Duhem-Quine thesis reminds us that *sub specie aeternitatis* there are many ways of doing that. Contrary to what the so-called pessimistic meta-induction would have us believe, however, the history of science is hardly a discontinuous

53. Maxwell was elected fellow at Trinity in 1855 and was thoroughly familiar with the writings of its Headmaster Whewell (Harman 1998, p. 32).



series of radically different basic taxonomies. On the contrary, when viewed from this perspective, it shows a remarkable continuity.

The much-maligned Hypothetico-Deductive (*H-D*) account of confirmation can be remedied with a little help from my *COIs*. It has often been claimed that the *H-D* account originated in the work of Herschel and Whewell. In a brief history of scientific methodology, based on Laudan (1981), van Fraassen, for instance, writes: "The first stage of my rough history is the debate over the rival accounts of *the methods of hypotheses* and *the inductive method*, between the (roughly classified) heirs of Descartes and Newton, respectively. The second stage is the synthesis of the two accounts, generally credited to Herschel and Whewell. (If we wish the title of hypothetico-deductive method to be honorific, it is probably best to identify it with their account, despite the barren wastes of later textbook presentations under that heading . . .)" (van Fraassen 1985, p. 263). This leads him to comment a few pages later: "It looks rather disingenuous now, but Whewell . . . claims the new method of hypotheses with frills to be the true explication of Newton's method of induction" (Ibid., p. 267). Van Fraassen is in good company when he pegs Whewell as a Hypothetico-Deductivist *avant la lettre*. Robert E. Butts, editor of the anthology from which all Whewell quotations in this section were taken, did the same in a misguided attempt to make Whewell the precursor of the "barren wastes" of the *H-D* method, which in the late 1960s could still be mistaken for state-of-the-art philosophy of science (see, e.g., Butts 1968, p. 17). So did renowned Darwin scholar Michael Ruse a few decades ago (Ruse 1975, p. 167).<sup>54</sup>

The crucial difference between this *H-D* picture of Whewell and the (*M*)*COI* picture that I propose concerns the relation between the data *E* and the hypothesis *H* corroborated by *E*. In the *H-D* picture, the relation is one of entailment, in the *COI* picture it is one of explanation. And with the demise of the Deductive-Nomological (*D-N*) or covering law model of explanation, it has become a platitude that "*H* entails *E*" cannot be substituted for "*H* explains *E*."

54. Unlike Whewell, Herschel can with some justification be seen as the father of hypothetico-deductivism. An important difference between Whewell and Herschel is that Herschel does not put any constraints on the kind of hypothesis that can be subjected to *H-D*-type testing. Any wild guess will do. For Whewell, by contrast, a hypothesis needs to have a considerable amount of explanatory power before it can even begin to be confirmed through *H-D*-type testing. In his otherwise very positive review of Herschel 1830, Whewell berated Herschel for his "spirit of gratuitous theorizing," and approvingly quoted Adam Sedgwick, who lectured: "the records of mankind offer no single instances of any great physical truth anticipated by mere guesses and conjectures" (Snyder 1997a).

When we stop reading Whewell through the lenses of *H-D* confirmation and *D-N* explanation, we recognize, with Darwin, that Whewell was not the least bit “disingenuous” in claiming that he was explicating Newton’s method of induction. Modern accounts of Whewell reflect this trend (see in particular Snyder 1997a, b; see also Losee 2001, pp. 108–115).

Once we recognize the difference between the *H-D* account and the (*M*)*COI* account of confirmation, we can use the latter to repair the former. The crucial move is to replace the relation “*H* entails *E*” in the *H-D* account by the relation “*H* explains *E*” with common-origin explanations providing an important example of the type of explanation involved. In addition, *H* should be allowed to stand for models and ideas, no matter how poorly articulated, not just for carefully formulated hypotheses. In this manner, we can salvage one happy outcome of the union of *H-D* confirmation and *D-N* explanation, namely the insight that confirmation is often just the flip-side of explanation.

After everything that has been said so far, it will be easy to recognize Whewell’s influence in the following passage from the *Origin*. It is a paragraph from the closing chapter of the sixth and final edition of 1872:

It can hardly be supposed that a false theory would explain, in so satisfactory a manner as does the theory of natural selection the several large classes of facts above specified. It has recently been objected that this is an unsafe method of arguing. But it is a method used in judging of the common events of life, and has often been used by the greatest natural philosophers. The undulatory theory of light has thus been arrived at; and the belief in the revolution of the earth on its own axis was until lately supported by hardly any direct evidence. It is no valid objection that science as yet throws no light on the far higher problem of the essence or origin of life. Who can explain what is the essence of the attraction of gravity? No one now objects to following out the results consequent on this unknown element of attraction (Darwin 1958, p. 452).

The first sentence echoes Whewell’s statement that “No false supposition could after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and un contemplated” (Butts 1968, p. 153). The fact that Darwin’s theory reduces so many seemingly disparate “classes of facts” to the same causal mechanism, i.e., evolution by natural selection, gives it—to use another phrase of Whewell that we encountered earlier—“a stamp of truth beyond the power of ingenuity to counterfeit.” The final part of Darwin’s paragraph (“It is no valid objection . . . element of attraction”) suggests that its

author was much more sensitive to Kepler's caveat than Whewell. Darwin is careful not to overreach. His results have thus proven to be remarkably robust. He admits candidly that he is not offering a finished theory. His project is forward-engaged.

The passage also displays Darwin's supreme confidence in the power of his argument. He is unfazed by the long list of objections to his theory that he patiently deals with in the *Origin* without being able to answer them fully. The age of the Earth, gaps in the fossil record, the lack of a mechanism for variation were all serious obstacles for Darwin's theory. Nevertheless, Darwin realized that his argument was strong enough to overcome the scientific objections and all theological ones as well.

The passage furthermore shows that Darwin improved considerably on Whewell's justification of the use of *COIs* and *MCOIs*. Humanity did not have to wait for the spectacular successes of Newton's applications of these types of inferences to recognize their value. As Darwin notes, "it is a method used in judging of the common events of life." Natural selection would have given short shrift to an evolutionary cousin of *Homo sapiens* that had somehow developed a tendency *not* to rely on *COIs* in navigating the hazards of prehistoric life. It is true that for *COIs* to be successful, the environment in which one draws such inferences must meet stringent conditions. Copernicus, Kepler, and Newton, in effect, tried to identify those when they attributed simplicity, unity, uniformity, and ontological parsimony to nature. But whatever those conditions are, ordinary experience makes it abundantly clear that they are satisfied in the homely environment of our everyday lives. It remains an open question whether the same can be said about the very small, the very large, or the very distant (either in time or in space), so that scientists can rely on *COIs* when theorizing about these foreign parts. The rich harvest of scientific results brought in so far with the help of *COIs*, however, makes it eminently reasonable to proceed as if one can. Never mind the occasional bad crop; this is the course of action one would recommend on the basis of another battle-tested principle of human experience: *if it ain't broke, don't fix it*.

Like Newton's gravitational theory, Darwin's theory provided strong guidance for subsequent research and thereby important safeguards against garden paths. This is especially clear in the field of botany.<sup>55</sup> Botanist Joseph Dalton Hooker was the first to be privy to Darwin's secret. Hooker was no instant convert, but he was not dogmatic about the fixity of species either. Whether species were fixed or mutable was largely a mat-

55. For the following I relied heavily on lecture notes on the Darwinian revolution kindly made available to me by Rich Bellon. For more details see Bellon (2000), especially chs. 6 and 7.

ter of expediency to him. He subscribed to whichever view made for better botany. As he wrote to Darwin on September 14, 1845: "I have no formed opinion of my own on the subject [of species], I argue for immutability, till I see cause to take a fixed post . . . & thus you see I am ever ready to make it subservient to Botany instead of Botany to it, as must be the true relation" (Burkhardt and Smith 1988, p. 254; quoted and discussed in Bellon 2000, pp. 223–225).

Hooker laid out his arguments for the permanence of plant species in the introduction of his two-volume *Flora Novæ-Zelandiæ*, the second part of *The Botany of the Antarctic Voyage of H.M. Discovery Ships 'Erebus' and 'Terror'* published in 1853.<sup>56</sup> As Darwin was writing the *Origin*, Hooker revisited the species question for the introduction to his *Flora Tasmaniæ*, the third and final part of *The Botany of the Antarctic Voyage*, published in 1860. Referring to the new ideas of Darwin and Alfred Russel Wallace published in 1858, Hooker argued that the time had come to change horses:<sup>57</sup>

[I]f, as I have endeavoured to show, all those attributes of organic life which are involved in the study of classification, representation, and distribution, and which are barren facts under the theory of special creations, may receive a rational explanation under another theory, it is to this latter that the naturalist should look for the means of penetrating the mystery which envelopes the history of species, holding himself ready to lay it down when it shall prove as useless for the further advance of science, as the long serviceable theory of special creations . . . now appears to me to be (Hooker 1860, p. xxvi).

Darwin himself made good on the promise of evolution for botany with his 1862 book, *On the Various Contrivances by Which British and Foreign Orchids Are Fertilized by Insects, and on the Good Effect of Intercrossing*. In a letter to his American confidant Asa Gray, Darwin described his *Orchids* as a "flank movement on the enemy" (Ghiselin 1969, p. 136; Browne 2002, p. 174; Bellon 2000, Ch. 7). While Huxley was promoting evolution in the arena of public opinion, the *Orchids* convinced a whole new generation of British botanists that evolution did indeed help one become a better botanist. Especially impressive was Darwin's solution to a riddle that had stumped some of the best minds in the field, namely the case of

56. The relevant passage can be found in Appleman (1979, pp. 15–18).

57. I am grateful to Rich Bellon for alerting me to this very telling passage which—just as tellingly—is not included in the excerpt of this text in Appleman (1979, pp. 227–231).

one particular kind of plant with a stem with what botanists took to be flowers of three different genera. Using an evolutionary explanatory scheme, Darwin showed that these flowers, in fact, correspond to the male, female, and hermaphrodite form of one and the same species (Browne 2002, p. 183; Bellon 2000, Ch. 7). Evolution could thus bypass theological and philosophical objections by providing the tools for solving otherwise intractable scientific problems.

Darwin's "flank movement" suggests that the *Origin's* mighty MCOI may have made its persuasive force felt indirectly. In terms of the distinction I made in the introduction, the commitment of evolutionary botanists, it seems, was more to the tools Darwin provided for classifying plants than to his general model for representing their evolutionary development.

**COI Story III: Special Relativity: "Asymmetries Which Do Not Appear To Be Inherent in the Phenomena"<sup>58</sup>**

In COI Story I, we saw that Copernicus could piece together his heliocentric model of the solar system from models already developed by Regiomontanus working in the Ptolemaic tradition. When Einstein developed his special theory of relativity in the early years of the last century, he did not have to start from scratch either. The formulae relating the space and time coordinates of different observers moving with respect to one another in relativistic space-time had already been found by H. A. Lorentz, after whom Poincaré subsequently named them. These Lorentz transformations are at the core of special relativity.

In the course of work in the 1890s on the electrodynamics of moving bodies—the area of physics that culminated in special relativity and that gave the 1905 paper in which Einstein introduced the theory its title—Lorentz discovered, working with Newtonian notions of space and time, that Maxwell's equations for electric and magnetic fields essentially retain their form under Lorentz transformations and not, as one would expect in Newtonian space and time, under what are now called Galilean transformations. In the usual terminology, Maxwell's equations are Lorentz invariant, not Galilean invariant.

With the help of this purely mathematical result, Lorentz was able to derive a peculiar physical effect. Consider an optical system consisting of an arrangement of optical components (lamps, mirrors, lenses, screens, etc.) and the pattern of light and shadow it produces. Suppose the system is initially at rest in the ether—the medium thought to carry electric

58. See Janssen (2002) for a concise historical reconstruction of this episode along the lines of this COI story.

fields, magnetic fields and electromagnetic waves. Now the system is set in motion with some velocity  $v$  with respect to the ether. Lorentz used the Lorentz invariance of Maxwell's equations to show that the pattern of light and shadow in the moving system will experience a minute contraction in the direction of motion by a factor  $(1 - v^2/c^2)^{1/2}$  (where  $c$  is the velocity of light).

In Lorentz's theory there is a sharp distinction between matter and field. Matter is governed by Newton's laws, which are Galilean invariant. Electric and magnetic fields, and thus patterns of light and shadow, are governed by Maxwell's equations, which are Lorentz invariant. Only systems governed by Lorentz-invariant laws are subject to the contraction effect. So in Lorentz's theory the pattern of light and shadow in the moving system contracts, but its material part does not.

As the Earth rotates on its own axis and orbits around the sun, the velocity with which an optical system in any terrestrial laboratory is moving with respect to the ether changes continuously. If the pattern of light and shadow contracts in whatever direction the system happens to be moving, while the optical components do not, one would expect to see the bands of light and shadow move across the screen over the course of a day and over the course of a year.

No experiment had ever detected such changes. To account for these negative results, Lorentz made a radically new physical assumption. He introduced a number of amendments to the laws governing matter so that the laws effectively governing matter become Lorentz invariant, just like Maxwell's equations governing the fields. That means that both matter and field components of a moving optical system will contract. Changes in the velocity of the system will then no longer have any observable effect, in agreement with the negative experimental results.

Lorentz's amended theory is empirically perfectly adequate. From an explanatory point of view, however, it is highly unsatisfactory. Lorentz invariance was supposed to be a peculiarity of Maxwell's equations. Now it turns out to be a property of the laws effectively governing matter as well. Put differently, it is an odd coincidence that any physical system—no matter what it is made of: steel, wood, or electromagnetic fields—contracts the exact same way when set in motion with respect to the ether. This coincidence cries out for an explanation and can thus be used as fodder for a *COI*.

After *COI Stories I* and *II*, it will be clear what the upshot of this *COI* will be: any acceptable theory must make one structure responsible for the Lorentz invariance of both the laws governing fields and the laws governing matter. Such a theory will then automatically explain the puzzling universal contraction of systems in motion. As in the case of Copernicus'

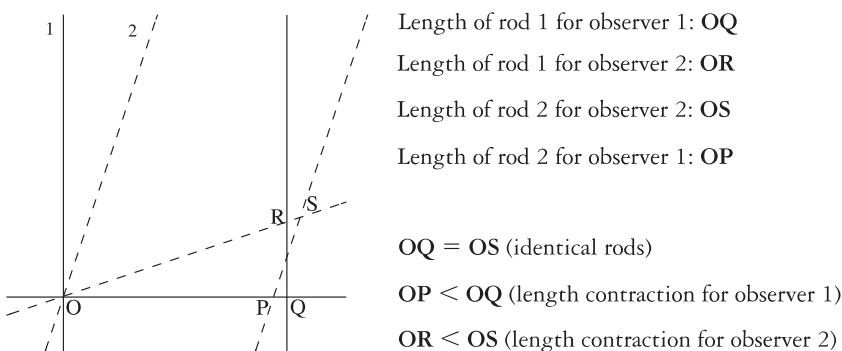
*COI* two such theories, both strongly forward-engaged, were given serious consideration in the early 1900s. One was special relativity, the other goes by the highfalutin name of the electromagnetic world-picture. According to this electromagnetic program, the common origin underlying universal Lorentz invariance is that all matter is made of electromagnetic fields and is thus governed by Maxwell's equations. Since Maxwell's equations are Lorentz invariant, all systems in motion must contract. The program initially showed promise, but did not pan out. Special relativity thus carried the day, but not thanks to this or any other *COI*. Variants of the *COI* laid out above, in conjunction with arguments from nascent quantum theory, removed Lorentz's theory from serious consideration, but did not decide between special relativity and the electromagnetic program.

According to special relativity the common origin of universal Lorentz invariance is the new space-time structure posited by the theory, known today as Minkowski space-time after the mathematician Hermann Minkowski who by 1908 had worked out its geometry. In Minkowski space-time, the spatio-temporal coordinates of different observers are related by Lorentz transformations rather than Galilean transformations. Any laws for systems in Minkowski space-time must accordingly be Lorentz invariant rather than Galilean invariant. The Newtonian laws that Lorentz used to describe matter are thus inadmissible in special relativity. It does not follow that matter must therefore be described by Maxwell's equations, as was the creed of the electromagnetic world-picture. In principle, any Lorentz-invariant laws will do. Special relativity is completely agnostic about the ultimate constitution of matter. The electromagnetic program, by comparison, went out on a limb, which proved to be its undoing.

Early adherents of the program, such as Max Abraham, tacitly assumed that space and time were Newtonian. Given that Maxwell's equations are Lorentz invariant, however, it is more natural to assume that space and time are Minkowskian. Minkowski once made a sneer to the effect that having Maxwell's equations govern fields in a Newtonian space-time is like "going to a concert with your ears stopped up with cotton wool" (Miller 1998, p. 330). Later proponents of the electromagnetic program, such as Gustav Mie, did adopt Minkowski space-time and generalized Maxwell's equations. The grand new world-picture was thus reduced to one among many possible special-relativistic (i.e., Lorentz-invariant) theories. In the 1920s, if not much earlier, it became clear that not everything in the world can be constructed out of electromagnetic fields and the program faded into obscurity.

Fig. 5 illustrates how Minkowski was able to trace the contraction of all physical systems to the structure of Minkowski space-time, the common origin of this universal phenomenon in special relativity. The two solid





**Figure 5.** Length contraction as a perspectival effect in Minkowski space-time.

lines through  $O$  are the space-time axes of observer 1. The vertical line is his time axis, the horizontal line is his space axis (two spatial dimensions need to be suppressed to represent four-dimensional space-time on a two-dimensional piece of paper).<sup>59</sup> The two vertical lines track the positions of the front and rear end of a rod at rest with respect to observer 1 through space-time. Such tracks are called worldlines. The two parallel tilted lines are the worldlines of the ends of an identical rod moving to the right with respect to observer 1, but at rest with respect to observer 2. The two dashed lines through  $O$  are her space-time axes. Her time axis is tilted because she is moving with respect to observer 1. Her space axis is tilted because she disagrees with observer 1 over which lines connect simultaneous events. Both observers will take the length of the line segments between the front and rear ends of the two rods on their own space axes as the lengths of these rods. As can be checked in Fig. 5, they will both conclude that the other person's rod is shorter than their own. This phenomenon is called length contraction in special relativity. According to special relativity, the universal contraction effect is thus akin to perspectival effects in ordinary space.<sup>60</sup>

59. Even so the figure does not capture all features of Minkowski space-time because its geometry is different from the Euclidean geometry of the paper. Both the two solid lines through  $O$  and the two dashed lines through  $O$ , for instance, are perpendicular to each other in Minkowski space-time, but the dashed lines are not perpendicular to each other in the figure. Similarly, the line segments  $OQ$  and  $OS$  are equally long in Minkowski space-time, but  $OS$  is longer than  $OQ$  in the figure.

60. Lorentz never really appreciated the purely kinematical Einstein-Minkowski explanation of length contraction (Dorling 1968, p. 68; Janssen 1995, sec. 4.3). This situation is reminiscent of the one we encountered in the work of the sixteenth century astronomer Clavius who failed to appreciate Copernicus' kinematical explanation of phenomena like retrograde motion (see 28 above).



What makes the special-relativistic explanation of Lorentz invariance of physical laws a good explanation? Is it because it provides a unified account or because it provides a causal account? In other words, does the example support Salmon's causal account of explanation or Friedman and Kitcher's unification account?

If unification were all that matters, it is unclear why Lorentz's theory does not provide a perfectly adequate explanation of length contraction. Lorentz's claim that all laws are Lorentz invariant has tremendous unifying power. On the basis of this one claim it can account for everything special relativity can account for. Yet, the theory is unsatisfactory from an explanatory point of view. It does not have the resources to explain why physical systems in the Galilean space-time posited by the theory are all governed by Lorentz-invariant laws. In short, Lorentz's theory unifies but does not explain. This explanatory deficiency was keenly felt by Lorentz's contemporaries. Poincaré (1906) wrote: "We cannot content ourselves with simply juxtaposing formulas that would agree only by some happy coincidence; the formulas should, so to say, penetrate each other" (quoted and discussed in Darrigol 2000, p. 366).

At first glance, Salmon's causal account is unsatisfactory as well. Minkowski space-time certainly explains length contraction, but it hardly qualifies as a causally efficacious substance. This objection can be avoided by broadening Salmon's concept of causation in the way I suggested in the introduction. The *COI* in this case is to a structure rather than a substance.<sup>61</sup> In this way Salmon gets this example right: special relativity explains and Lorentz's theory does not.

There are other morals to be drawn from this *COI* story. First, it provides another example of theory change driven by explanatory deficiencies (in the form of unexplained coincidences) rather than by empirical anomalies. Secondly, it captures the rationality of preferring special relativity (or the electromagnetic world-view when that was still a viable option) over Lorentz's theory in a way that transcends the local context of the episode. Thirdly, as in the case of Copernicus versus Ptolemy, the role of *COIs* in this episode shows how one can pick between theories of vastly different scope and in different stages of their development following a pattern of argumentation that once again transcends local contexts. In *COI* Story I, Copernicus was cast in the role of David and the Ptolemaic-Aristotelian tradition in the role of Goliath. Here the conjunction of Einstein's relativistic picture of space and time and any number of promissory notes is up

61. See Balashov and Janssen (forthcoming) for a defense of the claim that the *COI* to Minkowski space-time is independent of one's stance on the ontology of space-time (substantival or relational).

against Lorentz's comprehensive theory of matter and fields based on Maxwell's equations and Newtonian mechanics. In both cases, *COIs* exposed fatal (explanatory) weaknesses of the Goliath.

As in *COI Stories I and II*, some of the principal actors in the story used and discussed *COIs* themselves, as I will illustrate with quotations from Poincaré, Lorentz, Einstein, and Minkowski.

It will come as no surprise after his part in *COI Story I* that Poincaré was acutely aware of the power of *COIs*. In Poincaré's 1906 paper (completed before he read Einstein's 1905 paper), we find the following remarkable passage:<sup>62</sup>

Let us imagine some astronomer before Copernicus, pondering upon the Ptolemaic system. He would notice that, for every planet, either the epicycle or the deferent is traversed in the same time. This cannot be due to chance, and there must be some mysterious bond between all the planets of the system.

Then Copernicus, by a simple change of co-ordinate axes which were supposed fixed, did away with this seeming relationship: every planet described one circular orbit only . . .

Now, there may be an analogy with our problem. If we assume the relativity postulate, we find a quantity common to the law of gravitation and the laws of electromagnetism, and this quantity is the velocity of light; and this same quantity appears in every other force, of whatever origin. There can only be two explanations.

Either everything in the universe is of electromagnetic origin; or, this constituent which appears common to all the phenomena of physics has no real existence, but arises from our method of measurement . . .

Perhaps the abandonment of [the old method of measurement] would suffice to overthrow Lorentz's theory as decisively as the system of Ptolemy was by the work of Copernicus (Poincaré 1906; translation from Kilmister 1970, pp. 148–149).

Poincaré recognizes that there can be two *COIs* in the case at hand: one to the electromagnetic world-view and one to something reminiscent of, but not quite the same as, special relativity. In fact, Poincaré died a bitter opponent of special relativity in 1912.

Lorentz was more gracious. He realized that he was up against a power-

62. I quoted this passage in my dissertation (Janssen 1995, p. 282) and then forgot about it, but I clearly owe the idea of comparing the case of Einstein versus Lorentz to the case of Copernicus versus Ptolemy in Janssen (2002) to Poincaré. Note that Poincaré does not mention the role of Tycho in the Copernicus-Ptolemy case (neither did I in Janssen 2002).

ful argument but the argument was not strong enough to overcome his objections to special relativity. Einstein, he wrote, “may certainly take credit for making us see in the negative result of experiments like those of Michelson . . . not a fortuitous compensation of opposing effects but the manifestation of a general and fundamental principle. Yet, I think, something may also be claimed in favour of [Lorentz’s own] theory. I cannot but regard the ether, which can be the seat of an electromagnetic field with its energy and its vibrations, as endowed with a certain degree of substantiality” (Lorentz 1916, p. 230). This is the position with which Lorentz went to his grave in 1928.

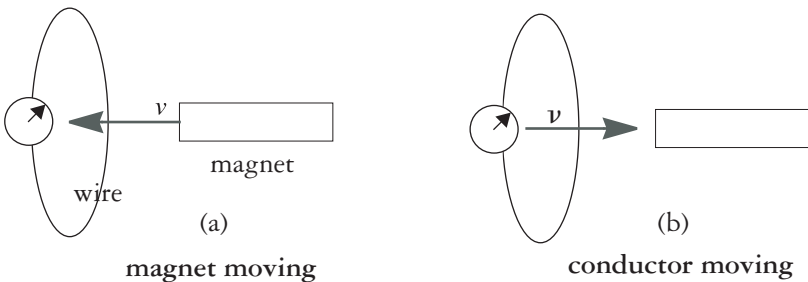


Figure 6. Einstein’s magnet-conductor example.

Einstein opens his famous 1905 paper, “On the Electrodynamics of Moving Bodies,” with a *COI*. In the very first sentence, he writes: “Maxwell’s electrodynamics . . . when applied to moving bodies, leads to asymmetries which do not appear to be inherent in the phenomena” (Einstein et al. 1952, p. 37). The example Einstein gives of such asymmetries is shown in Fig. 6. Consider a magnet and a conductor consisting of a piece of wire hooked up to an ammeter. Suppose magnet and conductor are moving with respect to one another at relative velocity  $v$ . In Lorentz’s theory we need to distinguish two cases. In case (a), shown on the left, the magnet is moving with respect to the ether. In case (b), shown on the right, the conductor is moving with respect to the ether.

In case (a), the magnetic field of the moving magnet will be changing in time at every point. As Faraday’s law of induction tells us, this changing magnetic field induces an electric field. The electric field produces a current in the wire, which will be registered by the ammeter. In case (b), the magnetic field is not changing in time anywhere and there will be no induced electric field. The free electrons moving with the wire, however, will experience the Lorentz force exerted by the magnetic field. As a result, the electrons will go in circles through the wire. This constitutes a current which will be registered by the ammeter.

Lorentz's theory predicts the exact same current for these two cases, even though one is due to an electric field and the other is due to a magnetic field. Einstein exploited this odd coincidence for *COI* purposes. The two cases, he argued, are really one and the same situation, looked at from different points of view. The example is used by Einstein to introduce his relativity postulate: only the relative velocity of the magnet and the conductor matters, the velocities with respect to the ether that Lorentz ascribed to them are completely meaningless. Before Einstein is through with his brief introduction, he has dismissed the ether as superfluous. This requires a major overhaul of nineteenth century physics. But like Copernicus' *COI*, Einstein's *COI* provided strong enough evidence that he was on the right track to sustain such efforts.

Another consequence of Einstein's relativistic interpretation of the situation in Fig. 6 is that electric fields and magnetic fields can no longer be conceived of as two entities existing side-by-side. There can only be one electromagnetic field, which breaks down differently into electric and magnetic components for different observers. From the point of view of the conductor in Fig. 6, the electromagnetic field has both a magnetic and an electric component, whereas from the point of view of the magnet it is purely a magnetic field. Einstein emphasized this point in 1919 in a long article intended for *Nature* but never actually submitted (Pais 1982, pp. 177–178):

In the formulation of the special theory of relativity, a consideration—not mentioned so far—concerning Faraday's electromagnetic induction played a leading role [Einstein gives magnet-conductor example]. The idea that we would be dealing here with two fundamentally different situations was unbearable to me. . . . The existence of the electric field was therefore a relative one, dependent on the coordinate system used, and only the electric and magnetic field taken *together* could be ascribed some kind of objective reality. This phenomenon of electromagnetic induction forced me to postulate the . . . relativity principle (Janssen et al. 2002, pp. 264–265).

The juxtaposition of this 1919 passage with the passage from the 1905 paper above nicely illustrates that Einstein used *COIs* both in the context of pursuit (if we take him at his word in this 1919 article) and in the context of persuasion.

Einstein's magnet-conductor example is also discussed in Miller (2002) on Einstein and Picasso. As much as I enjoyed reading this book, I strongly disagree with the way in which it connects relativity and Cubism. To quote one sympathetic reviewer: "Miller's idea is not just to tell us

[the story of Einstein, Picasso, and the changes they wrought in our conceptions of space and time] in side-by-side biographies, but also to search for evidence of common roots that raise these events above mere coincidence" (Goodstein 2002, p. 247). In a paper on *COIs* it is good to remind ourselves every once in while of the truism that sometimes coincidences are just coincidences. In my opinion, Picasso and Cubism are completely irrelevant to understanding the development of special relativity.

Miller writes about the magnet-conductor example that Einstein "is pointing out something artificial, redundant, and *therefore unaesthetic*" (Miller 2001, p. 197, my emphasis).<sup>63</sup> In other words, the asymmetry in Lorentz's explanation of the situation in Fig. 6 is bad because it offends Einstein's aesthetic sensibilities. Miller's assessment thus makes Einstein's argument highly context-dependent. To quote another sympathetic reviewer: "The purpose of both science and art is to discover the reality that lies behind the appearances. This reality must, of course, conform to the highest aesthetic standards" (Brush 2001, p. 50). I hate to start sounding like a broken record, but my *COI* reconstruction of Einstein's use of the magnet-conductor example shows that the force of his argument transcends the context in which it was originally put forward and is *a fortiori* completely independent of the broader cultural setting analyzed in *Einstein and Picasso*.

Special relativity not only merges the electric and the magnetic field into one electromagnetic field, it also merges space and time into space-time, and energy and momentum into energy-momentum. The upshot of Einstein's *COI*, like so many other *COIs* we have meanwhile encountered, is thus that the new theory posits one structure to account for phenomena that were attributed to various structures in the old one.

Einstein was careful not to overreach. Special relativity, unlike the electromagnetic program, does not make any claims about the constitution of matter. The theory only imposes the constraint that all physical laws be Lorentz invariant. The reason Einstein did not want to commit himself to anything more specific was that he recognized that physics was in for another major overhaul, this one coming from quantum phenomena.

Einstein felt strongly, however, that a theory would only be satisfactory if it identified the structures responsible for the phenomena. As he wrote to Arnold Sommerfeld in 1908: "A physical theory can be satisfactory only if its structures are composed of elementary foundations. The theory of rel-

63. Kosso (1999) addresses the harmful but common practice among modern physicists of casting arguments from explanatory power in the form of appeals to aesthetics. As Martin Klein has suggested to me, the late Einstein's extreme rationalist pronouncements (see Norton 2000) may well be at the root of this unfortunate trend.

ativity is ultimately as little satisfactory as . . . thermodynamics was before Boltzmann had interpreted the entropy as probability” (quoted in Calaprice 2000, pp. 230–231).

In 1919, Einstein introduced the terms “constructive theories” and “theories of principle” to capture this distinction (Einstein 1954, p. 228; cf. Janssen et al. 2002, pp. 206–215). For Einstein special relativity was a theory of principle. With the introduction of Minkowski space-time, however, it became a constructive theory. Minkowski space-time is the structure responsible for all the effects derivable from special relativity alone. Special relativity, from this point of view, replaced Newtonian space and time by Minkowski space-time and does not make any claims about the contents of the new space-time other than that their spatio-temporal behavior had better accord with Minkowski’s new rules.

Section I of Minkowski’s famous 1908 talk, “Space and Time,” also begins with a *COI*: “The equations of Newton’s mechanics exhibit a two-fold invariance. Their form remains unaltered, firstly, if we subject the underlying system of spatial co-ordinates to any arbitrary change of position; secondly, if we change its state of motion . . . [T]he two groups . . . lead their lives entirely apart. . . . But it is precisely when they are compounded that the complete group as a whole gives us to think” (Einstein et al. 1952, pp. 75–76). Minkowski’s argument can easily and without distortion be translated into *COI* language. In Newtonian theory it is a coincidence that the laws of nature are invariant both under rotations of the spatial axes and under Galilean transformations to uniformly moving frames of reference. Special relativity explains this coincidence: the laws of nature are invariant under Lorentz transformations and those include both rotations of the spatial axes and transformations to uniformly moving frames of reference (which reduce to Galilean transformations for small velocities).

Minkowski makes it clear that he is using this argument in the context of persuasion. At several points, he mentions how mathematicians *might* have arrived at special relativity via this *COI*. At the outset, he says: “I should like to show *how it might be possible*, setting out from the accepted mechanics of the present day, along a purely mathematical line of thought, to arrive at changed ideas about space and time” (Einstein et al. 1952, p. 75, my emphasis). Later on in the lecture he elaborates: “The thought might have struck some mathematician [that the Lorentz group is more natural than the Galilean group]. Such a premonition would have been an extraordinary triumph of pure mathematics. Well, mathematics, though it now can display only staircase-wit [*Treppenwitz*], has the satisfaction of being wise after the event” (Einstein et al. 1952, p. 79).

Minkowski died in 1909, two years before Einstein finally came to

appreciate the tremendous value of the four-dimensional geometrical formulation of special relativity. Initially, the patent clerk had dismissed the priceless contribution of his former math teacher, whose classes he had skipped, as “superfluous learnedness” (Pais 1982, p. 152).

The three separate *COIs* considered in this section (my own, Einstein’s, and Minkowski’s) can be combined to form one big *MCOI* to Minkowski space-time, which then makes for a strong argument for preferring the modern relativistic interpretation of Minkowski space-time over the (neo-)Lorentzian one.

#### ***COI Story IV: General Relativity: “The Happiest Thought of My Life”***

Einstein was not satisfied with special relativity for very long. Only two years later, he was ready to move on. He describes the crucial moment in his 1919 article for *Nature*, in the passage immediately following the one quoted in the preceding section:<sup>64</sup>

Then came to me the happiest thought of my life in the following form.

Like the electric field generated by electromagnetic induction, the gravitational field only has a relative existence. *Because, for an observer freely falling from the roof of a house, no gravitational field exists while he is falling . . .*

The experimental fact that the acceleration due to gravity does not depend on the material is thus a powerful argument for extending the relativity postulate to systems in non-uniform relative motion with respect to one another (Pais 1982, p. 178; see also Janssen et al. 2002, p. 265).

Einstein’s *COI* is thus from the equality of inertial and (passive) gravitational mass to an extension of the relativity principle from uniform to accelerated motion. I am afraid that the argument as it stands is a *non-sequitur*. By pointing to the striking coincidence that two completely different concepts in Newtonian theory, resistance to acceleration and susceptibility to gravity, have the exact same numerical value, Einstein certainly put his finger on a serious explanatory deficiency in Newton’s theory, but the *COI* from this coincidence to the relativity of arbitrary motion is unwarranted. The legitimate *COI* takes us from the coincidence, inertial mass equals gravitational mass, to the claim that in any satisfactory theory inertia and gravity must be attributed to one and the same structure.

64. This passage is also analyzed in *Einstein and Picasso*. Miller (2001, pp. 216–220) once again appeals to aesthetic considerations to make sense of Einstein’s argument (cf. my comments on his analysis of the magnet-conductor example in *COI Story III*).

Einstein, in fact, comes close to getting it right in the first sentence of the second paragraph. The magnet-conductor example, Einstein points out, has shown that the induced electric field in this case has only a “relative existence.” Only the electromagnetic field has “objective existence,” where objective means independent of what frame of reference is chosen, that of the magnet or that of the conductor. In the frame of the magnet the electromagnetic field has only magnetic components. In the frame of the conductor, this same electromagnetic field has both magnetic and electric components. What is true of electric and magnetic fields is true of inertia and gravity as well. Only the “inertio-gravitational field” has objective, i.e., frame-independent, reality. This inertio-gravitational field is represented by curved space-time in general relativity. For an observer moving on a geodesic (i.e., a straightest line in what in general will be a crooked space-time) the inertio-gravitational field in his or her vicinity only has an inertial component. For an observer not moving on a geodesic this same inertio-gravitational field has both an inertial and a gravitational component. An observer falling freely in the inertio-gravitational field, such as the person falling off the roof in Einstein’s example, is moving on a geodesic. An observer resisting the pull of gravity, say on the surface of the earth, or accelerating, say in some rocket ship, is moving on a non-geodesic. All these observers can claim to be at rest if they want. In that rather trivial sense, the introduction of an inertio-gravitational field splitting differently into inertial and gravitational components allows one to talk about an extension of the relativity principle from uniform to arbitrary motion. But this general relativity principle is different from the relativity principle for uniform motion (see, e.g., Dorling 1978). That principle says that for two observers in uniform relative motion, it makes no sense to ask who is moving and who is at rest. It is impossible to tell the difference. For two observers in relative motion in some arbitrarily curved space-time, one moving on a geodesic, one moving on a non-geodesic, it is easy to tell which one is which.

So Einstein was overreaching with his *COI*. He wanted to kill two birds with one stone. The stone in this case is the insight, which Einstein dubbed the “equivalence principle,” that inertia and gravity are of the exact same nature (“*Wesensgleich*”).<sup>65</sup> The birds are the unexplained equality of inertial and gravitational mass and the privileged inertial frames of Newtonian theory. The stone killed the first bird, but not the second. General relativity is a misnomer for Einstein’s theory of the inertio-gravitational field. There are still preferred states of motion in the theory,

65. For Einstein’s definitive formulation of the equivalence principle and references to the extensive literature on this topic, see Janssen et al. (2002, pp. 37–44).



namely motion along geodesics. Such preferred states, however, are not determined by Newton's absolute space or Minkowski's absolute space-time, with the unphysical property of acting upon matter (by giving it its marching orders) without being acted upon. In general relativity, matter curves space-time and thus contributes significantly to which paths will be geodesics and which ones will not. That eventually reconciled Einstein with the absolute motion he had not been able to eradicate with general relativity.

At least one person in the late-nineteenth century clearly recognized the explanatory deficiency of Newton's theory exploited in the *COI* that set Einstein on a royal road to general relativity. Heinrich Hertz pointed out in a lecture series at the University of Kiel in 1884:<sup>66</sup> "But we do want to make it clear, that the proportionality of [gravitational] mass and inertia calls for an explanation just as much as the equality of the velocities of electrical and optical waves, and that it is as unacceptable in the former case as in the latter to present it as meaningless" (Fölsing 1999, p. 122).

Einstein was not nearly as good as Darwin at articulating the principles of his own reasoning. We have seen him use two strong *COIs*, one to get special relativity, and one to get general relativity off the ground. We also noted an important difference between the two cases. In the case of special relativity, Einstein was careful not to move beyond what can legitimately be inferred from the empirical evidence. This approach corresponds to what he later called a theory of principle. In the case of general relativity, Einstein was not that careful. The equivalence principle and such constraints as energy-momentum conservation and compatibility with the old Newtonian theory in the case of weak static fields provided him with a solid empirical basis, but he also relied on what amounts to nothing more than a prejudice, namely that all motion must be relative. At various points, he also used the kind of appeals to mathematical elegance that he routinely made in his later years (Norton 2000).

Tragically, Einstein seems to have lost his ability to come up with good *COIs* as he got older. His preference for constructive theories over theories of principle was probably an important factor in this development. Another factor was the strong vision he had in his later years of what an acceptable constructive theory should look like. In his work on unified field theory, he was guided almost exclusively by this vision and by considerations of mathematical elegance. He stopped paying attention to the empirical data. In 1932, for instance, the miracle year of nuclear physics, Einstein was busy vacillating between three unified field theories extending

66. I am grateful to Jürgen Renn for alerting me to this passage.

general relativity in different directions. In light of this, it may not come as a surprise that he was never able to articulate the rule of inference that had led him to so many of the successes of his youth.

In his autobiographical notes of 1949, Einstein gives fascinating accounts of many of these successes. Reading these passages, the primed reader is hit over the head with *COIs*. Yet, before Einstein gets into describing his discoveries, he apologizes for not being able to tell the reader how he did it:

The meager precision of the assertions in the last two paragraphs [with some ruminations on scientific methodology] I shall not attempt to excuse by lack of sufficient printing space at my disposal, but confess herewith that I am not, without more ado [immediately], and perhaps not at all, capable to replace these hints by more precise definitions. I believe, however, that a sharper formulation would be possible. In any case it turns out that among the “augurs” there usually is agreement in judging the “inner perfection” of the theories and even more so concerning the “degree” of “external confirmation” (Einstein 1949, p. 23).

The tentativeness of this passage—note the copious use of scare quotes in the last sentence—is highly uncharacteristic for Einstein who typically comes across as a supremely confident and sure-footed writer.

Even in his earlier statements, Einstein does not preach what he practiced, which was to take advantage of the evidentiary value of explanatory power. In 1919, the same year that he formulated the important distinction between a theory of principle and a constructive theory, Einstein threw up his hands when it came to the question of how to pick between empirically equivalent theories: “If two theories are compatible with all the data, then there is no other criterion for the preference of one over the other than the intuitive sense of the scientist. In this way one can understand that intelligent scientists with a command of the theories and the data can be passionate proponents of opposite theories” (Janssen et al. 2002, p. 219). The year before, on the occasion of Planck’s sixtieth birthday, his answer had been different but equally disappointing: “The development [of physics] has shown that of all the conceivable theoretical constructions one always proves to be absolutely superior to all others” (Einstein 1954, p. 226; cf. Janssen et al. 2002, pp. 54–59).

Fortunately, we can console ourselves with the advice Einstein gave in 1933: “If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: do not listen to their words, fix your attention on their deeds” (quoted and discussed in Norton 2000, p. 136).

Let me return once more to “the happiest thought” of Einstein’s life. When one corrects for Einstein’s unwarranted prejudice about the nature of motion, the *COI* he mounted on the basis of the equality of inertial and gravitational mass produced one of his most enduring contributions to physics. The upshot of this *COI* is that we shall never go back to a theory in which gravity and space-time are represented by two different structures. This basic insight of general relativity is much more secure than the edifice Einstein built on it. I agree with Steven Weinberg when he writes:

I don’t see any reason why anyone today would take Einstein’s general theory of relativity seriously as the foundation of a quantum theory of gravitation, if by Einstein’s theory is meant a theory with a Lagrangian density given by just the [Riemann curvature scalar which does not contain any derivatives of the metric higher than of second order]. It seems to me that there is no reason in the world to suppose that the Lagrangian does not contain all the higher terms with more factors of the curvature and/or more derivatives, all of which are suppressed by inverse powers of the Planck mass, and of course don’t show up at any energy far below the Planck mass, much less in astronomy or particle physics. Why would anyone suppose that these higher terms are absent (Weinberg 1999, p. 247)?

In Einstein’s defense, it should be pointed out that, unlike latter-day relativists, he was not wedded to the field equations that Weinberg is talking about. Between 1913 and 1919, Einstein published six different sets of field equations, two of them after he published the generally-covariant Einstein field equations in November 1915. In the first paper on his metric theory of gravity, he already made the point that Weinberg makes in the passage quoted above, albeit in a different context: “A priori one cannot deny the possibility that the final exact equations of gravitation could be of higher than second order” (Einstein and Grossmann 1913, p. 11).

Notice that Weinberg makes a conditional statement: “If by Einstein’s theory is meant a theory with [the Riemann curvature scalar as the] Lagrangian density,” then there is no “reason [to] take Einstein’s general theory of relativity seriously as the foundation of a quantum theory of gravitation.” But we should not throw out the baby with the bath water. It would be an unprecedented reversal of fortunes in the history of science if a satisfactory theory of quantum gravity could be found by quantizing some gauge field in flat Minkowski space-time as physicists tried to do for several decades. Most researchers in the field today recognize this. So the equivalence principle—formulated along the lines of Einstein’s definition in 1918: space-time and gravity are to be represented by the same struc-

ture—is a more lasting contribution to science than the Einstein field equations.

I have often heard physicists ask philosophers and historians of science whether they have any advice for them. Invariably, philosophers and historians either stonewall or rehearse a canned speech to the effect that the question betrays a fundamental misunderstanding of the nature and the goals of their discipline. No advice is ever forthcoming. It may therefore be somewhat presumptuous on my part to recommend that scientists try their hands at *COIs*. Given the example of quantizing gravity I just mentioned and given that some of the greatest scientists of the past—Newton, Darwin, and Einstein among them—made such liberal use of *COIs*, I would say that it is worth a try.

### Conclusions

Below I have brought together what I consider to be the most important conclusions of this first foray into the land of *COI*:

1. *COIs* exploit the epistemic value of explanatory power (*pace* van Fraassen). *Corollary 1.1*: *COIs* drastically reduce Duhem-Quine underdetermination. *Corollary 1.2* (*pace* Kuhn): *COIs* can account for theory change driven by explanatory deficiencies rather than by empirical anomalies.<sup>67</sup>
2. *COIs* can establish beyond the shadow of a doubt that the phenomena they tie together will be represented by one and the same structure in any future theory. *Corollary 2.1*: *COIs* can overcome weighty objections. *Corollary 2.2*: *COIs* often involve forward-engagement. *Corollary 2.3*: *COIs* can decide between theo-

67. The development of quantum theory, however, was very much driven by empirical anomalies rather than by explanatory deficiencies of the old theory. In fact, the *new* theory was seen as explanatorily deficient in this case. Besides the work in general philosophy of science that they are most famous for, both Kuhn and van Fraassen worked extensively in the area of quantum theory—Kuhn on its history, van Fraassen on its philosophical foundations. Their intuitions about science in general are thus colored by what may be a rather atypical example of science.

Even in the case of quantum theory, however, *COI*-ish considerations played an important role. The evidentiary value of the black-body spectrum or the hydrogen spectrum for the development of quantum theory rests on the *COI*-ish argument that there had to be something right about the inconsistent models from which the formulae for these spectra could be “derived” by Planck and Bohr, respectively. The agreement between Planck’s formula and the empirical data was deemed too good to be a mere coincidence. The same was true for Bohr’s “derivation” of formulae for the hydrogen spectrum and the Rydberg constant. It was clear that both Planck and Bohr were on to something; it was much harder to pinpoint exactly what that was.

ries of vastly different scope and in different stages of their development.

3. *COIs* do not license claims about the ontological status of the inferred common origins. *Corollary* (*pace* Salmon): *COIs* cannot be used as an argument for scientific realism.
4. *COIs* typically are to causal networks, structures, or mechanisms. *Corollary*: Salmon's causal account of explanation looks more promising than the unification account of Kitcher and Friedman.
5. *COIs* capture an element of rationality in theory choice across disciplines, periods, and locales.

The study of *COIs* is a forward-engaged project itself. From the point of view of history of science, the most important task is to replace *COI* stories by serious historical narratives. From the point of view of philosophy of science, the following two questions are most pressing. First, what is wrong with the arguments of van Fraassen and others that explanation only has pragmatic value? Second, and this is the key question, what is the force behind *COIs*?

Before distilling what little can be learned from the *COI* stories related in this paper about the general justification of this type of inference, I should emphasize that the induction of *COI* into the canons of scientific reasoning need not be postponed until this thorny issue has been settled. As I mentioned in the introduction and as I illustrated with the example of evolutionary botany, theories typically provide both representations of reality and tools for further investigation of reality. If *COIs* along with the broad taxonomic claims made in their conclusions suggest tools for refining that taxonomy, a scientist does not need any further justification to start using those tools. So, the reliance on *COIs* in what I called the context of pursuit does not call for any general philosophical justification. The issue of the justification of *COI* as a pattern of inductive inference only comes up in the context of persuasion, where what is at stake is no longer just the question whether a new taxonomy revealed by some *COI* is fruitful but whether it cuts nature at the joints. This last question is the hard one and some may be tempted to short-circuit it by embracing the notion that it is fruitfulness all the way down and that truth has nothing to do with it. That would be in the spirit of Hooker, but not in the spirit of Darwin; nor, for that matter, in the spirit of Copernicus, Kepler, Newton, and Einstein.

The best justification that we have come across so far for *COIs* as a pattern of inductive inference comes from Darwin. For Darwin they were ultimately just an extension of everyday reasoning. This requires us to

assume that whatever conditions account for *COI*'s high success rate in our neck of the woods also hold in the strange new worlds explored by science. The inductive risk of deploying *COIs* to extend the frontiers of science is thus twofold. First, there is the ordinary risk associated with any ampliative inference. Second, there is the added risk of wielding *COIs* on battle fields where we have absolutely no guarantee that the conditions for their deployment hold. Can we do no better than this?

I am afraid the answer is no. Consider what a stronger justification for the deployment of *COIs* at the frontiers of science would look like. We somehow need to establish that possible worlds without coincidences are vastly more probable than worlds with such coincidences. This requires the appropriate a priori probability distribution over possible worlds. Even so one could defeat the argument by making Maxwell's demon lord of the universe. On rebuttal one could quote Einstein's famous dictum, "Subtle is the Lord, but malicious he is not." But we do not even have to go that far to see that this whole line of reasoning is a *reductio* of such an attempt to justify the use of *COIs*. Philosophy is not statistical mechanics.

Proponents of inflation are routinely confronted with this *reductio*. Bob Wald and Stefan Hollands have recently introduced a "blind-folded Creator" to deride the inflationary *COI*. After listing the "why"-questions with which Alan Guth exposed the explanatory deficiencies of standard big bang cosmology, they write: "The image that seems to underlie the posing of these questions is that of a blindfolded Creator throwing a dart towards a board of initial conditions for the universe. It is then quite puzzling how the dart managed to land on such special initial conditions" (Hollands and Wald 2002, p. 2). I agree with Hollands and Wald that this type of justification is unavailing.

What was good enough for Darwin, however, should be good enough for us. It is true that a *COI* is a risky proposition at the frontiers of science. But that is part of the game. All one can ask for is the presence of reasonable safeguards against garden paths. Along with proposing a new taxonomy, the scientist championing a *COI* had thus better tell us how the new taxonomy can be put to work so that, if not its truth, at least its fruitfulness can be ascertained. This condition is met in spades by evolutionists and inflationists (it is also where creationists come up woefully short). It seems to me that it is good news for philosophers and historians of science that *COIs* do not have better credentials. It is good news for philosophers because without such credentials *COIs* can hardly be invoked to settle philosophical disputes such as the debate over scientific realism. It is good news for historians because a scientist not swayed by the power of a *COI* does not automatically have to be brandished as irrational in their narratives.

Be that as it may, I hope to have convinced the reader that *COI* is an important pattern of scientific reasoning that is used in research traditions across disciplines, locales, and periods, and that richly deserves further attention from both historians and philosophers of science.

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